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ELECTRICAL POTENTIALS OF THE HUMAN BRAIN [1]

BY ALFRED L. LOOMIS, E. NEWTON HARVEY AND GARRET HOBART

(From the Loomis Laboratory, Tuxedo Park, N. Y., and the Physiological Laboratory, Princeton University)

INTRODUCTION

The work of Berger, Adrian, Kornmüller, Davis, Gibbs, Jasper, Foerster and others has established the existence of electrical potentials arising in the cerebral cortex of man which can be recorded from electrodes placed on the scalp. The importance of this discovery lies not in the fact that potentials can be detected, which is to be expected, but that extraordinarily regular rhythms of potential occur which differ in different individuals and are affected by various conditions of mental activity, by external stimuli, by emotional states, sleep, lack of oxygen, anaesthesia and pathological conditions in the brain.

As one examines a group of records of these rhythms one is confronted with a myriad of problems. One soon learns to distinguish many different types of activity. Typical examples are shown in Fig. 1. The question as to which part of the brain the different types come from, and the manner in which different stimuli affect the various types has only been partially answered. Space does not permit a review of the work which has been carried out in an attempt to solve these

[1] Preliminary reports of this work have been published in *Science*, **81**, 597, 1935; **82**, 198, 1935; **83**, 239, 1936.

problems. Many other characteristics of the records will undoubtedly become important besides these types of activity. The records must contain a mass of information of which only a small amount can be deciphered at the present time. This leads to a difficulty of nomenclature. How should one describe the various types of activity? Until one knows definitely the parts of the brain giving rise to the different types and the factors determining their appearance or disappearance, a logical system of nomenclature is impossible.

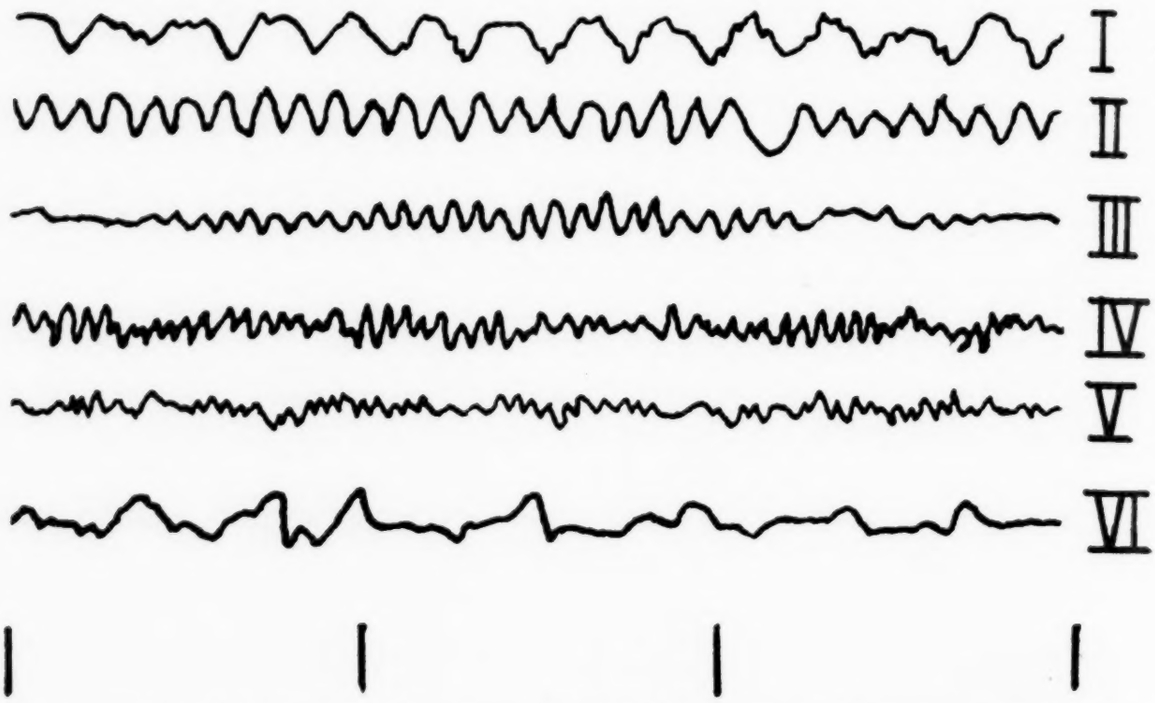

FIG. 1. Types of brain potential waves. Explanation in text. Lines at bottom of figure give seconds.

For this reason we have preferred to leave the question of nomenclature in abeyance until more is known about the localization of the potentials and the factors affecting them. In this paper we will refer merely to the different types as they appear on the records. These types merge into one another and no sharp line can be drawn between them.

Type I, called by us in previous papers 'Saw Toothed.' Large potential variations occurring 4 to 7 times a second but without regular frequency. Common in young children.

Type II, called 'Alpha Waves' by Berger. Adrian refers to them as 'The Berger Rhythm.' We have called them in

previous papers 'Trains' of waves. They are regular rhythms with definite frequencies, usually between 9 and 11 cycles per second. They appear in most adults, are readily affected by conditions of mental and emotional activity, and have been extensively studied.

Type III, called by us in previous papers 'Spindles.' Rhythms with a regular frequency between 12 and 15 cycles per second. They appear only during sleep.

Type IV. Rhythms between 20 and 24 a second with a definite frequency, rarely observed.

Type V, called 'Beta Waves' by Berger. Rapid variations in potential averaging from 25 to 35 changes per second.

Type VI. Large random activity with no measurable frequency.

Our work has been concerned with long time records of normal individuals between the ages of 17 days and 74 years, particularly the variation in individuals and the changes during sleep. Twenty-nine different persons have been studied, all for at least two hours, one for a total of 50 hours. We have also investigated a subject under hypnosis, another under the influence of alcohol, and an individual that had been blind since birth. Long time records have necessitated the development of an improved method of recording which could be not only continuous but also convenient for examination at any later time.

Technique

Three rooms are devoted to the brain potential experiments. The first (sleeping room) contains a space 8 feet wide × 11 long by 9 high, electrically shielded with copper netting, large enough for a bed, table, chairs, the first stages of three amplifiers and accessory apparatus used in the investigation. The second (amplifier room) contains the remaining stages of the brain amplifiers and the amplifiers used in recording heart rate, bed movement, respiration, and for sound communication between the sleeping room and the third or control room 66 feet away.

The control room contains the eight foot drum for recording the ink records, three cathode ray oscillographs and camera

for the photographic records, an oscillator for sending tones of various frequency to a loud speaker in the sleeping room and the various switches and relays for automatically sending stimuli to the subject.

The sleeping room is in a far corner of the laboratory away from general noise, and can be made completely dark and all signals can be electrically transmitted to the subject from the control room. A microphone near the bed picks up the slightest sound, even the deep breathing of a sleeper.

The brain potentials to be recorded vary from 1 to 100 microvolts and the frequencies from 2 to 40 per second. Three amplifiers are used so that potentials from three parts of the head may be recorded simultaneously. Each amplifier consists of four stages, capacity coupled with large condensers. 6C6 tubes are used in the first three stages and an 89 tube in the fourth stage. The output of each amplifier is applied through a potentiometer to a cathode ray oscillagraph. The three oscillagraphs are so arranged that the three records can be photographed simultaneously on standard motion picture film. The output of each amplifier is also applied through a potentiometer to the grid of a power tube (2A3) which operates one of the three high speed dynamic pens shown in Fig. 2. These trace curves on paper wrapped on a revolving drum. The drum, shown in Fig. 3, is a steel tube 8 feet long and 44 inches in circumference. It is driven by a synchronous motor and geared for either one half or one revolution per minute. For the higher speed each 1.46 inches of paper corresponds to a second. The pens are mounted on a platform which travels on a threaded bar so as to move the pens horizontally at the proper speed to describe a close spiral on the paper. One pen writes in red and the other two in blue ink.

A calibration curve is placed on every record in the following manner. A precise alternating potential whose frequency varies smoothly from 30 to 0 cycles per second is obtained by passing paralled light through a slit in front of a photocell, and modulating the light with a shutter shaped so as to form a sine wave. The potential drop across a resistance in

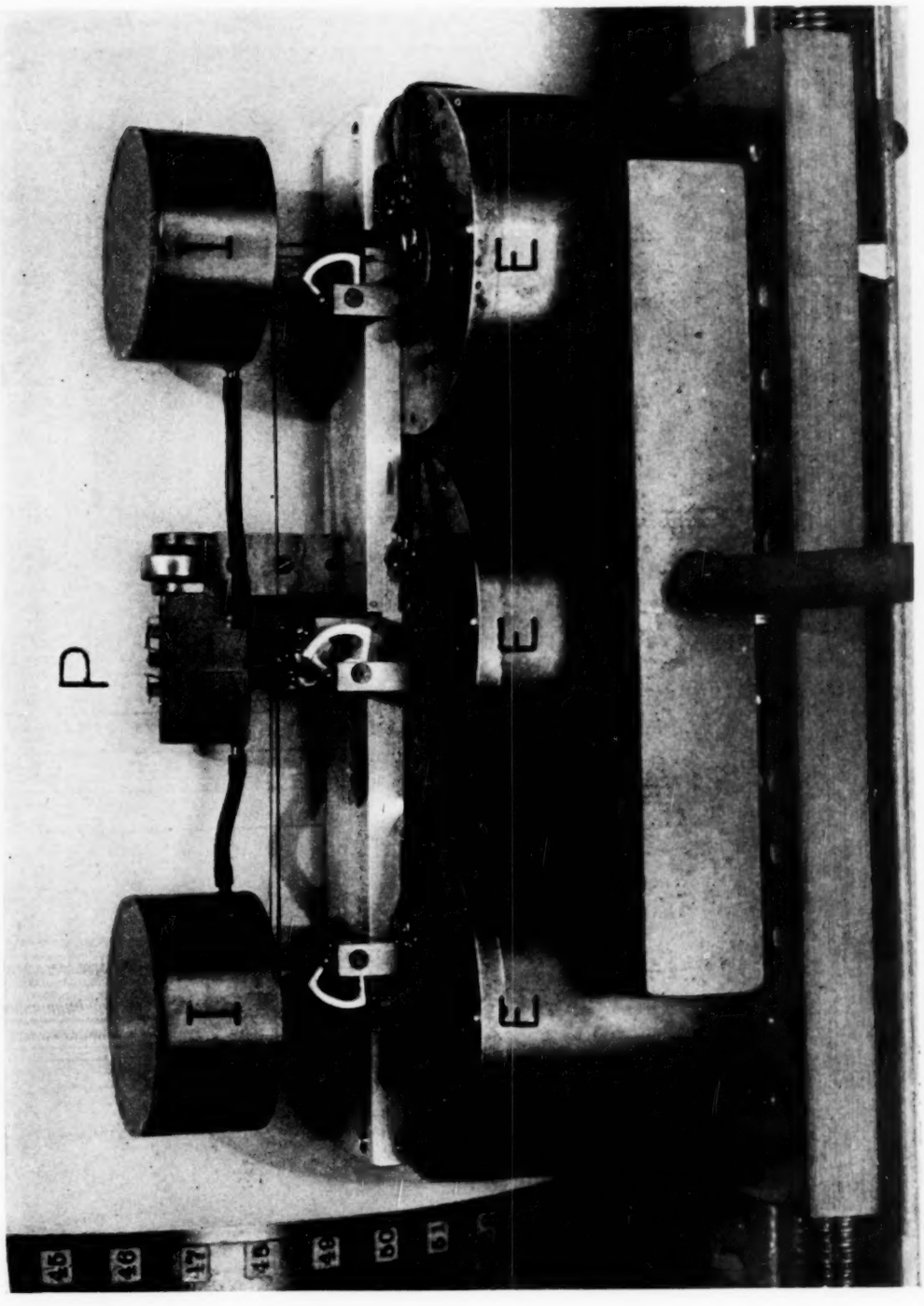

FIG. 2. Photograph of pens (*P*) in position to write on drum. *I*, ink reservoirs. *E*, electromagnets.

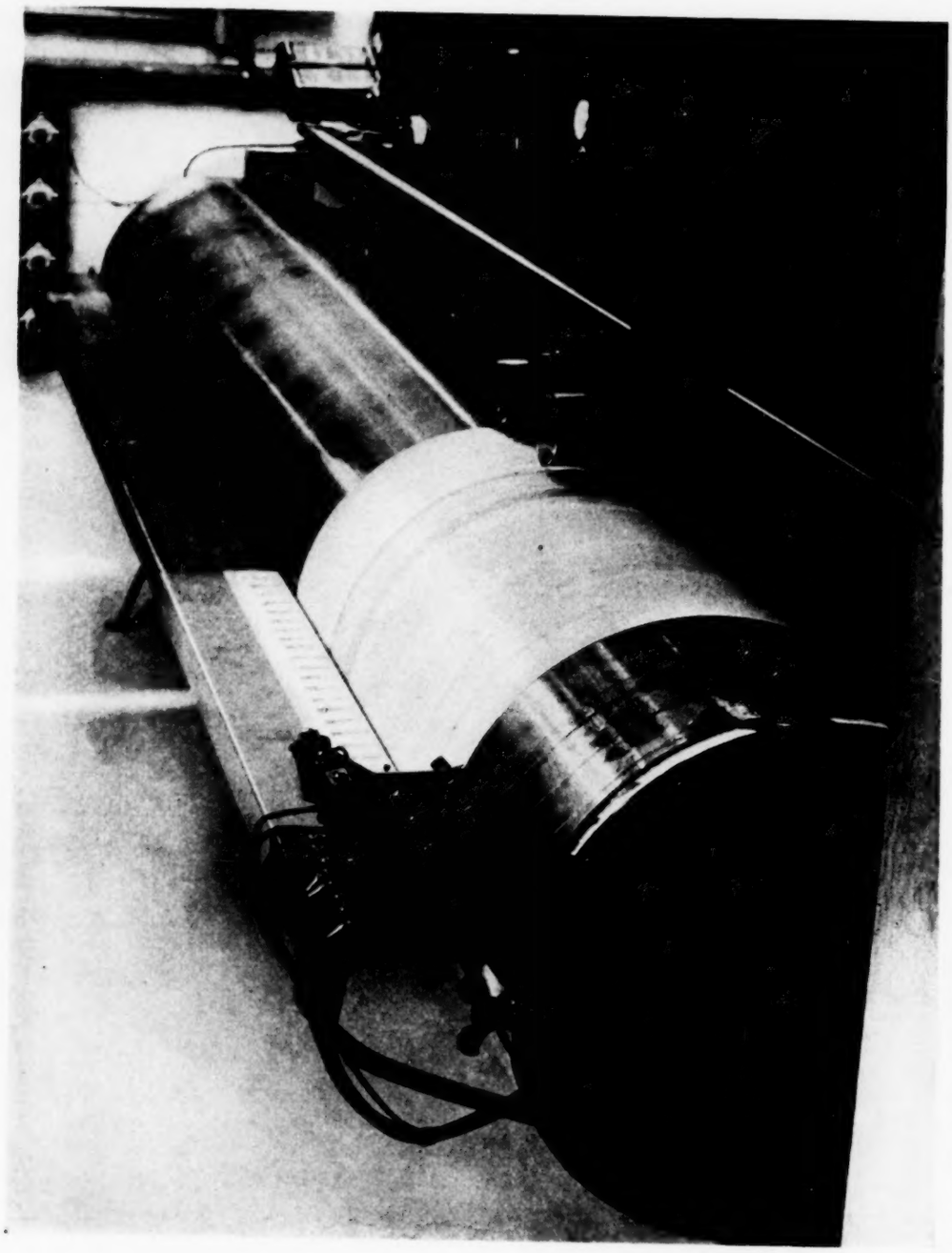

FIG. 3. Photograph of eight foot drum with pen carriage and rachet wheels (upper left) for integrating heart beats or brain potentials.

series with the photocell is calibrated so that 10, 20, 30, 40 or 50 microvolts can be applied to the input of the amplifiers. When a calibration is desired a synchronous motor with flywheel revolves the shutter 30 times a second, the power is shut off and the shutter slowly reduces speed coming to rest in about 25 seconds. The frequency at any instant is read directly from the record.

Simultaneous records taken with the three cathode ray oscillographs and the pen recorders, show that the pens give a sufficiently accurate reproduction of all the important potentials arising in the brain. (See Fig. 4*C*.) Since the drum is driven by a synchronous motor it acts as its own clock. By appropriate contacts sound or light or other stimuli may be sent to the subject with each revolution of the drum, thereby placing the response of the subject in the same relative position on the record and greatly simplifying the reading of a record and comparison with control periods when no stimulus was sent to the subject, as in Fig. 8*A*. Eight hours of record can be recorded on a sheet of paper 8 feet long and 44 inches wide, a great improvement over the use of ticker tape which would require a ribbon of paper over half a mile long. Perhaps the important point to emphasize in regard to our technique is the automatic control of all recording at a distance from the subject, who remains undisturbed throughout the experiment.

Auxiliary apparatus will also record on the paper the subjects heart rate and respiration rate, and whether the subject moved in bed or made any noise. The details of this auxiliary apparatus will not be described in this paper, as it was not used in the particular experiments here reported.

During the last months, however, we have developed two devices which enormously aid in the analysis and interpretation of the brain potential records. One is a resonant circuit which can be inserted when desired in the grid circuit of the third stages of one of the amplifiers. This permits the amplifier to be sharply tuned to any desired frequency. Figure 4*A* shows a calibration curve made when the tuned amplifier was successively tuned to 10, 11, 12 and 13 cycles per second.

Figure 8*A* shows a brain potential record in which the left pen recorded from an amplifier tuned to 10 cycles.

The other device is an 'Integrator' which is connected to the output of the tuned amplifier and records every thirty seconds the amount of activity of the brain at that frequency which had occurred during the preceding 30 seconds. The 'Integrator' consists of a 'neon flashing circuit' made up of a battery, a variable resistance, and a neon light across a condenser. The variable resistance consists of a 6C6 tube. The output of the tuned amplifier is applied through a potentiometer to the grid of this tube which is biased to cut off. If the brain is not producing any potentials of the frequency for which the amplifier is tuned, the tube is offering a very high resistance and the neon light will flash only a few times during the 30 seconds. As the brain activity increases, the resistance of the tube will decrease and the neon light will flash more often. The number of flashes of the neon light each 30 seconds is automatically counted and recorded on the drum. The rate of flashing is calibrated in terms of microvolts appearing at the input of the amplifier by the calibrating device referred to above. This integrator enables one to detect at once small changes in average brain activity which are not detectable in the pen records without laborious analysis.

Electrodes

Fortunately the type or character of the electrodes that are placed on the head does not seem to be critical.

Fig. 4

A. Response of amplifiers tuned to 10, 11, 12 and 13 cycles (right line) compared with calibration curve for 30 microvolts of varying frequency (left line). Lines between *A* and *B* give seconds.

B. Muscle potentials and artifacts. *A*, clenching jaws; 9, turning over in bed; 10, tongue moving in mouth (no effect); 11, winking eyes rapidly; 12, talking (little effect). Left line from front of head, right line from back. Time in seconds to left.

C. Cathode-ray oscillograph record of simultaneous potentials from occiput (left) crown (middle) and vertex (right) of head. Electrode position *A*. Time in seconds to right.

Read all records from top to bottom and from left to right.

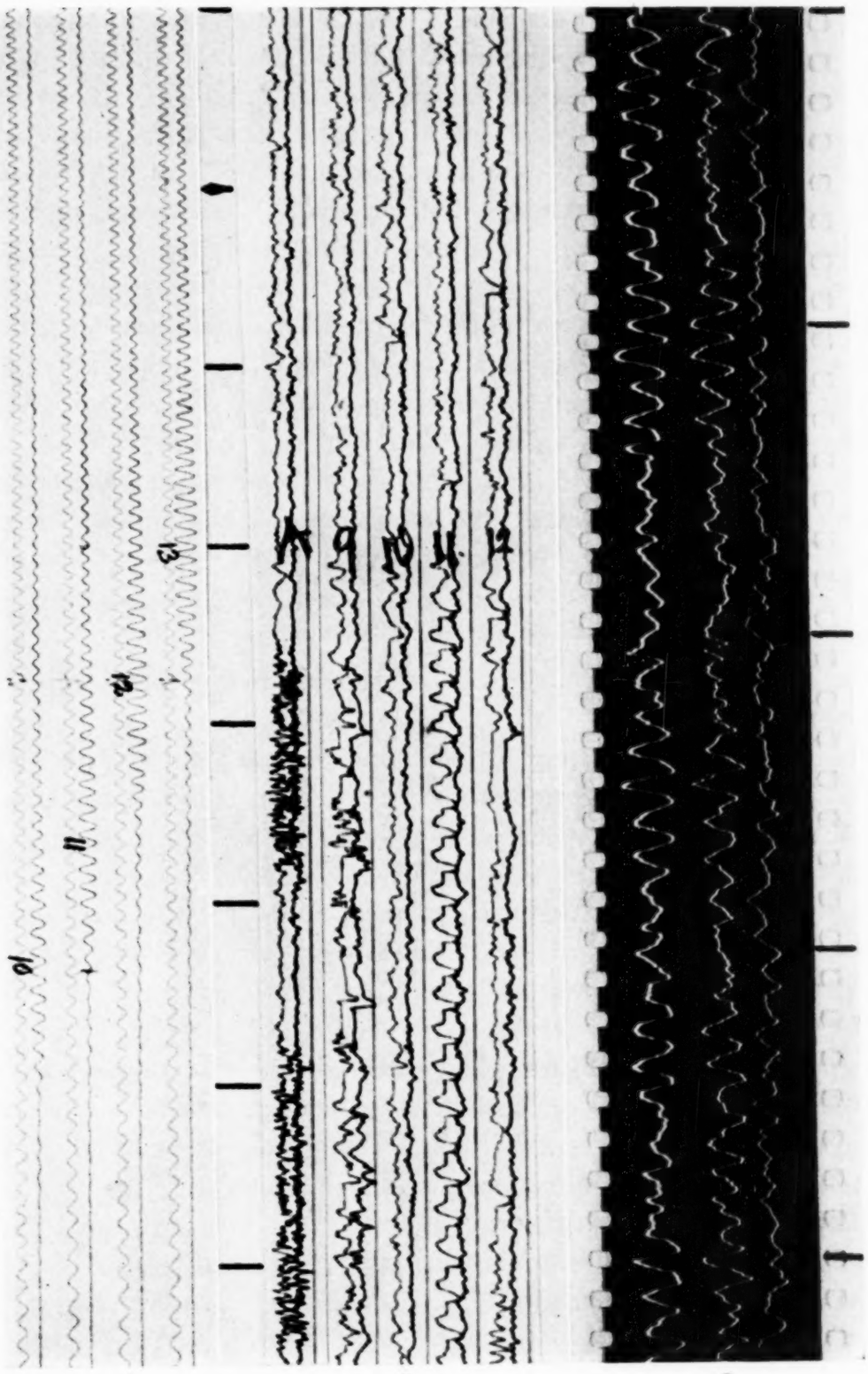

FIG. 4

We have used either rings of platinum 5/8 inch in diameter, covered with Sandborn electrode paste and held on the shaved scalp with adhesive tape, or small loops of silver wire covered with cotton soaked in 10 percent salt solution and held in place by a rubber band around the head. We have also used small platinum disks 4 mm in diameter held in place with a drop of collodian. The resistance of the electrodes seem to make little difference because of the high input impedance of the amplifiers. Tests with conventional 'nonpolarizable' electrodes seem to show that they offer no particular advantages and are inconvenient to use. Others have also used with success needles thrust through the skin.

Of course potentials due to dissimilar metals in contact with an electrolyte must be avoided and all the electrodes must be of the same metal during any one test.

Electrode Position

We have usually used one of the following five combinations.

Position (A). One grid electrode on the occiput, another on the crown and another on the vertex, with the ground electrode in the form of a ring around the head above the ears.

Position (B). Horizontally on the back of the head with one grid electrode on the occiput, another to its right and another to left of the occiput, again with the ground electrode in the form of a ring.

Position (C). Three electrodes in the mid-line on high forehead, crown and occiput using the crown for a ground electrode.

Position (D). Three electrodes horizontally across the occiputal region with the middle one the ground.

Position (E). One grid electrode on occiput, another on the vertex and two ground electrodes, one behind each ear.

Muscle Potentials and Movement Artefacts

Muscle potentials because of their peculiar character can readily be distinguished from the brain potentials. They

appear from electrodes on both front and back of the head when the jaws are clenched, or moved sideways, on swallowing or yawning or pressing tongue on roof of mouth. They are most marked from the front position when the forehead or nose is wrinkled, the eyes are moved in their sockets or the eyelids shut tight. They do not appear when the muscles of the arms or legs are contracted.

Each time the eye is winked a single continuous potential excursion lasting 1/10 second occurs from the front electrode only. The change shows no high frequency muscle potentials and probably represents a rotation of the eyeball whose retina is at a different potential from the cornea.

Violent movements of the subject or violent joggling of the electrodes gives rise to easily recognizable potentials and frequently the amplifiers become choked for a moment giving a disturbance in the record, so characteristic that it cannot be mistaken for brain potentials. Some muscle potentials and movement artifacts are shown in Fig. 4*B*.

Normal Adult Records

Records from different persons, awake with eyes closed, clearly differ in character. Extreme differences are to be observed in N. H. (Fig. 5*A*) and in J. D. (Fig. 5*B*). In the former, practically no alpha rhythm (Type II) appeared but instead a marked 20–24 cycle rhythm (Type IV) not abolished by visual impressions. In the latter a marked regular alpha rhythm appeared in each of the records taken on 18 different days over a period of a year. Generalizations are difficult to make but we have gained the impression that these differences are characteristic of the individual. Where a marked alpha rhythm appears the differences observed are in (1) frequency, (2) the amount of disturbance which abolishes the rhythm, (3) potentials superposed on the rhythm and (4) the response to various stimuli and mental and emotional states.

Typical examples of the frequencies of alpha waves in different subjects is given in Tables 1, 2, and 3. Table 1, which contains counts of 5400 waves of J. D. will indicate

the amount of variation to be expected from a single individual. Each figure is the average of 10 counts of 10 waves each (100 waves) measured at random. It will be observed from table 1 that the frequency is more or less characteristic of the individual. One point we have repeatedly observed in a number of different persons is a slightly slower rhythm upon waking after a sleep or a considerable period of rest. In J. D. the average change is about 5 percent. Since temperature affects the rhythm of an individual (Hoagland) the question arises as to whether these changes might be due to a change in body temperature. As the variation in human

TABLE 1

Alpha wave frequencies of J. D. 23 years. Each section represents determinations made on one day. The figures before and after sleep are waves per second

Record 1935	Before Sleep	After Sleep	Record 1935	Before Sleep	After Sleep	Record 1935	Before Sleep	After Sleep
6/11/35	9.98	9.00	8/31/35	9.71	8.98	11/10/35	9.79	
	9.81			9.61	9.13		9.86	
				9.69				
2/2/36	9.39			9.61		12/1/35	9.44	
8/27/35	9.84	9.51	9/18/35	10.16		12/8/35	9.25	
	9.98			10.00				
				10.02		12/16/35	9.11	
8/28/35	9.51			9.58			9.56	9.44
	9.61			9.63				
	9.36	9.49		9.63		1/8/36	9.39	
				9.53			9.89	
8/29/35	9.74			9.41			9.71	
	9.74							
	9.56		10/12/35	9.89	9.13	1/28/36	9.41	
	9.76						9.58	
			10/14/35	9.86	8.98		9.53	
8/30/35	9.41	9.27					9.51	9.09
	9.49	9.11	Average	9.74	9.05		9.89	9.16
Average	9.66	9.28				Average	9.57	9.23

FIG. 5

A. Record of N. H. Electrode position *C*; left line from high forehead, right from crown. At the horizontal line a neon light was flashed on. The horizontal dashes give reaction times. Note type IV waves, not abolished by light.

B. Record of J. D. Electrode position *E*; left line from vertex (tuned 10 cycles), middle line from vertex (untuned), right line from occiput (untuned). Note alpha rhythm from occiput.

Time in seconds between *A* and *B*. Read from top to bottom and from left to right. Lines at top separate revolutions of drum (1/2 min. apart).

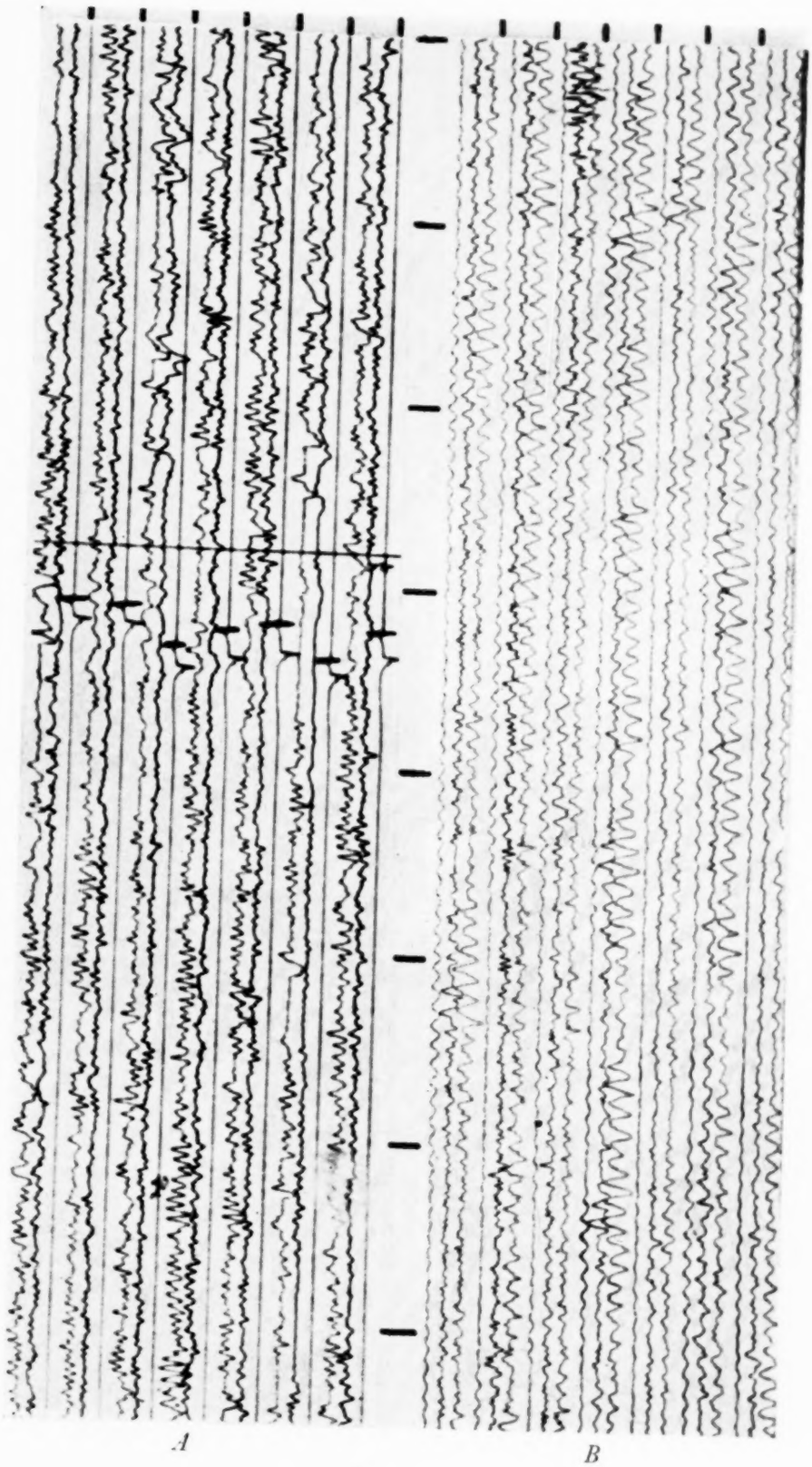

FIG. 5

the amount of variation to be expected from a single individual. Each figure is the average of 10 counts of 10 waves each (100 waves) measured at random. It will be observed from table 1 that the frequency is more or less characteristic of the individual. One point we have repeatedly observed in a number of different persons is a slightly slower rhythm upon waking after a sleep or a considerable period of rest. In J. D. the average change is about 5 percent. Since temperature affects the rhythm of an individual (Hoagland) the question arises as to whether these changes might be due to a change in body temperature. As the variation in human

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6/11/35	9.98	9.00	8/31/35	9.71	8.98	11/10/35	9.79	
	9.81			9.61	9.13		9.86	
				9.69				
2/2/36	9.39			9.61		12/1/35	9.44	
8/27/35	9.84	9.51	9/18/35	10.16		12/8/35	9.25	
	9.98			10.00				
				10.02		12/16/35	9.11	
8/28/35	9.51			9.58			9.56	9.44
	9.61			9.63				
	9.36	9.49		9.63		1/8/36	9.39	
				9.53			9.89	
8/29/35	9.74			9.41			9.71	
	9.74							
	9.56		10/12/35	9.89	9.13	1/28/36	9.41	
	9.76						9.58	
			10/14/35	9.86	8.98		9.53	
8/30/35	9.41	9.27					9.51	9.09
	9.49	9.11	Average	9.74	9.05		9.89	9.16
Average	9.66	9.28				Average	9.57	9.23

FIG. 5

A. Record of N. H. Electrode position *C*; left line from high forehead, right from crown. At the horizontal line a neon light was flashed on. The horizontal dashes give reaction times. Note type IV waves, not abolished by light.

B. Record of J. D. Electrode position *E*; left line from vertex (tuned 10 cycles), middle line from vertex (untuned), right line from occiput (untuned). Note alpha rhythm from occiput.

Time in seconds between *A* and *B*. Read from top to bottom and from left to right. Lines at top separate revolutions of drum (1/2 min. apart).

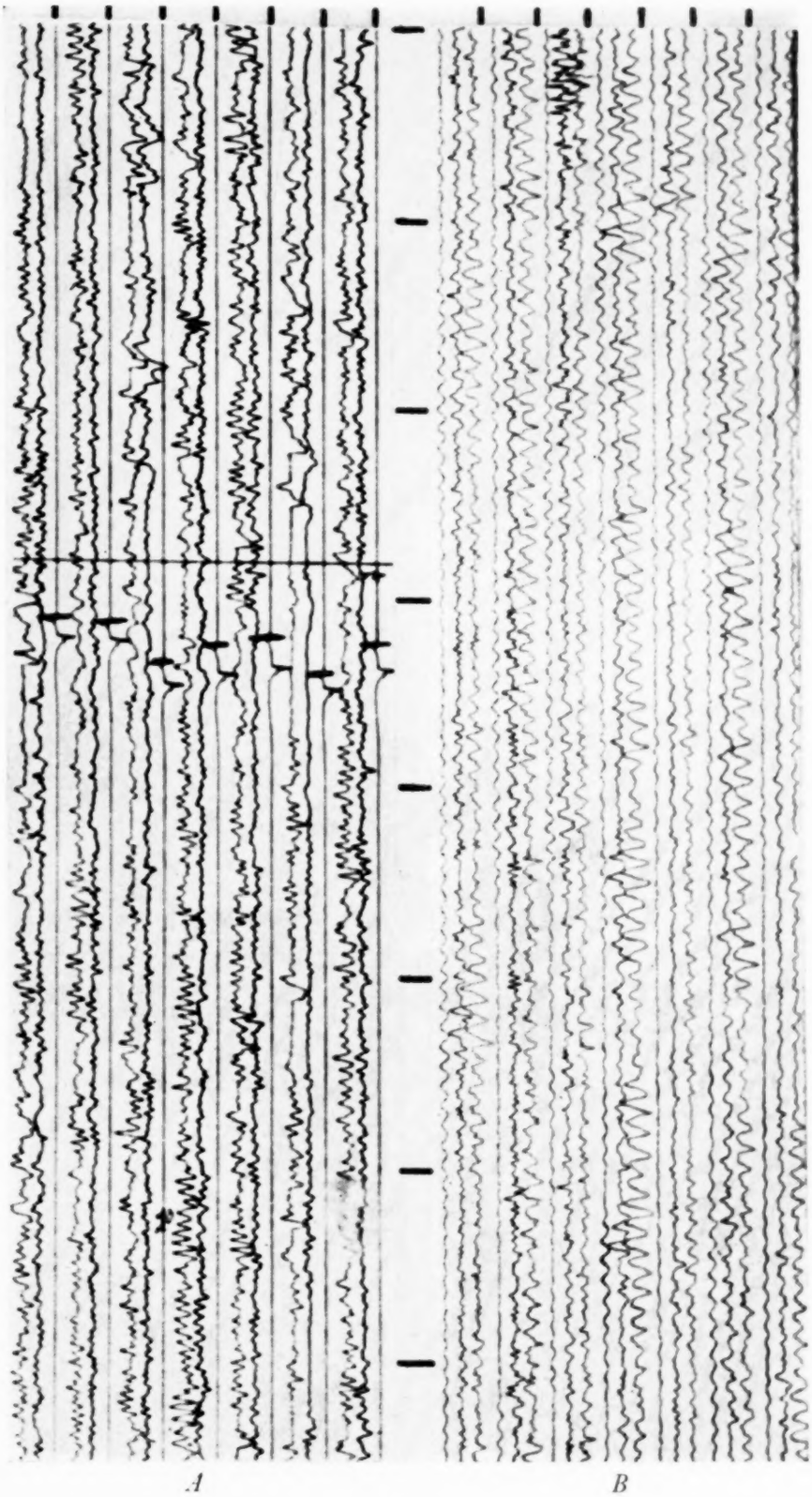

FIG. 5

TABLE 2

Alpha wave frequencies of other persons. Each section represents determinations made on one day. The figures in brackets give ages. The figures before and after sleep are waves per second. See also Table 3 for W. B.

Subject and Age	Before Sleep	After Sleep	Subject and Age	Before Sleep	After Sleep	Subject and Age	Before Sleep	After Sleep
H. L. (16)	9.94	9.84	M. C. (25)	12.54		P. M. (41)	9.46	
	9.94	9.66		12.70			9.51	
	9.71	9.66		12.07				
	9.76			11.78		P. M. (41)	9.69	
				12.00			9.39	
G. M. (16)	10.05	9.61		11.64			9.25	
	10.10	9.74		12.14	10.63		9.25	
	10.10			11.82	11.21			
						P. M. (41)	8.94	
L. M. (16)	8.65	7.71	G. H. (28)	9.82			8.81	
	8.72			9.78			8.81	
	8.33							
	8.30		W. R. (35)	10.97		R. S. (54)	9.66	
				10.63			9.39	
M. M.	10.85			10.70			9.16	
	10.88			10.88			9.74	
	10.06			11.05			9.46	
	10.92			10.75	10.57			
	10.92					E. R. (74)	9.63	9.13
			W. R. (35)	11.25			9.61	9.23
A. P. (32)	9.80	9.34		11.30				9.02
	9.84	9.79						

TABLE 3

Alpha wave frequencies of W. B. Hypnosis experiments

Record 1935	Before Hypnosis	During Hypnosis	After Hypnosis	Record 1935	Before Hypnosis	During Hypnosis	After Hypnosis
Oct. 26	10.05	9.76		Dec. 15	10.16	9.86	
		9.74	9.71			9.86	
						9.66	
Oct. 27	10.75	9.89				10.00	
						9.94	
Dec. 14	9.92	9.89				9.61	
	9.98	9.86					
	9.92			Average	10.16	9.82	
Average	10.10	9.83	9.71				

body temperature during sleep is only about 0.8° F. (Kleitman) and the temperature coefficient for brain rhythms is such that 3° F. increases the rhythm from 9 to 10, it appears unlikely that the change after sleep can be accounted for by changes in body temperature.

Disturbances which abolish the alpha rhythms in a subject will be discussed later. In a person lying undisturbed, emotional conditions and drowsiness are probably the most important and will obviously modify the picture in a record from one person from time to time, as well as in records from two different individuals.

Sleep

When a person retires for sleep the alpha waves continue while resting quietly but become less frequent while drowsing, *i.e.* the disturbances which abolish the rhythm become more and more frequent, even though the subject when asked says he is awake. At this time sounds heard by the subject, which tend to arouse him will give rise to continuous trains of waves. The exact moment of sleep is difficult to determine, if it can be said to have a meaning. The depth of sleep certainly varies, although a quantitive measure of depth is difficult.

In one experiment we used the following criterion of sleep. A readily audible sound, a click, was transmitted to the subject each minute and he was asked to say, 'awake,' if he heard the click. As the minutes passed it was noted that his reaction time to the click became more prolonged and finally no response was given. The brain potentials recorded during the experiment showed no sudden change in character and even when the breathing was slow and deep, the alpha waves kept coming through at intervals for about 15 minutes, gradually becoming less frequent, finally changing into 'random' potentials (Type VI) characteristic of sound sleep. At this time a cough outside the room, which did not awaken the sleeper, caused the alpha waves to appear again. No sudden change in character of record accompanies the onset of sleep but abrupt changes occur on awakening.[2]

We have however, observed in a number of sleepers a 13 to 15 cycle rhythm (Type III) appearing occasionally and lasting 1/2 to 1 second, which we have called 'spindles.' The frequency of these spindles has been as follows in different

[2] The random potentials cannot be said to have a frequency, but undoubtedly correspond to the '3 to 5 per second potentials' described by Gibbs, Davis and Lennox as characteristic of sleep.

persons: H. L., 13.7; M. C., 14.7; W. B., 12.5; J. D., 13.3 per second. Where electrode positions have been used which differentiate between top and back of head the type has been most marked from the top position, while the alpha rhythm is more marked from the back position (see Fig. 6).

During a night's sleep some periods may be practically devoid of waves while others will present a considerable number. On awakening the characteristic alpha waves appear again.

It is during sleep that the effect of sounds in starting the alpha rhythm becomes apparent. We gain the impression that the content of the sound is important, that it need not necessarily be loud, but that rustling of paper, low conversation, a cough, footsteps are most efficient. During deep sleep loud sounds may not be effective in starting the alpha rhythm with some persons. The effect would appear not to be a direct result of the sound stimulus but a change in the general level of brain activity (level of consciousness).

In two sleepers we have observed a peculiar type of irregular wave of large amplitude lasting from two to four minutes at a time when no movement or change in heart beat or respiration occurred, and in one of these cases observation of the sleeper confirmed the fact that he was sleeping quietly. Such disturbances may be connected with dreams but we have not yet with certainty correlated any change in the record with the onset of a dream.

During alcoholic stupor one of our subjects who had consumed 500 cc. of gin (212 cc. pure alcohol) within 30 minutes showed a marked large alpha rhythm with secondary potentials superposed on the regular rhythm quite different from the rhythm which had appeared before the alcohol was taken. At this stage the frequency of the alpha rhythm was very

FIG. 6

Record of J. D. showing typical spindles during sleep (to left of arrow) and alpha rhythm after awakening (to right of arrow). At arrow there is movement and muscle potential. Electrode position *A*; left line occiput, middle line crown, right line vertex. Note spindles on vertex and crown, alpha rhythm on occiput and crown.

Time in seconds to left. Read from top to bottom and from left to right. Lines at top separate revolutions of drum (1/2 min. apart).

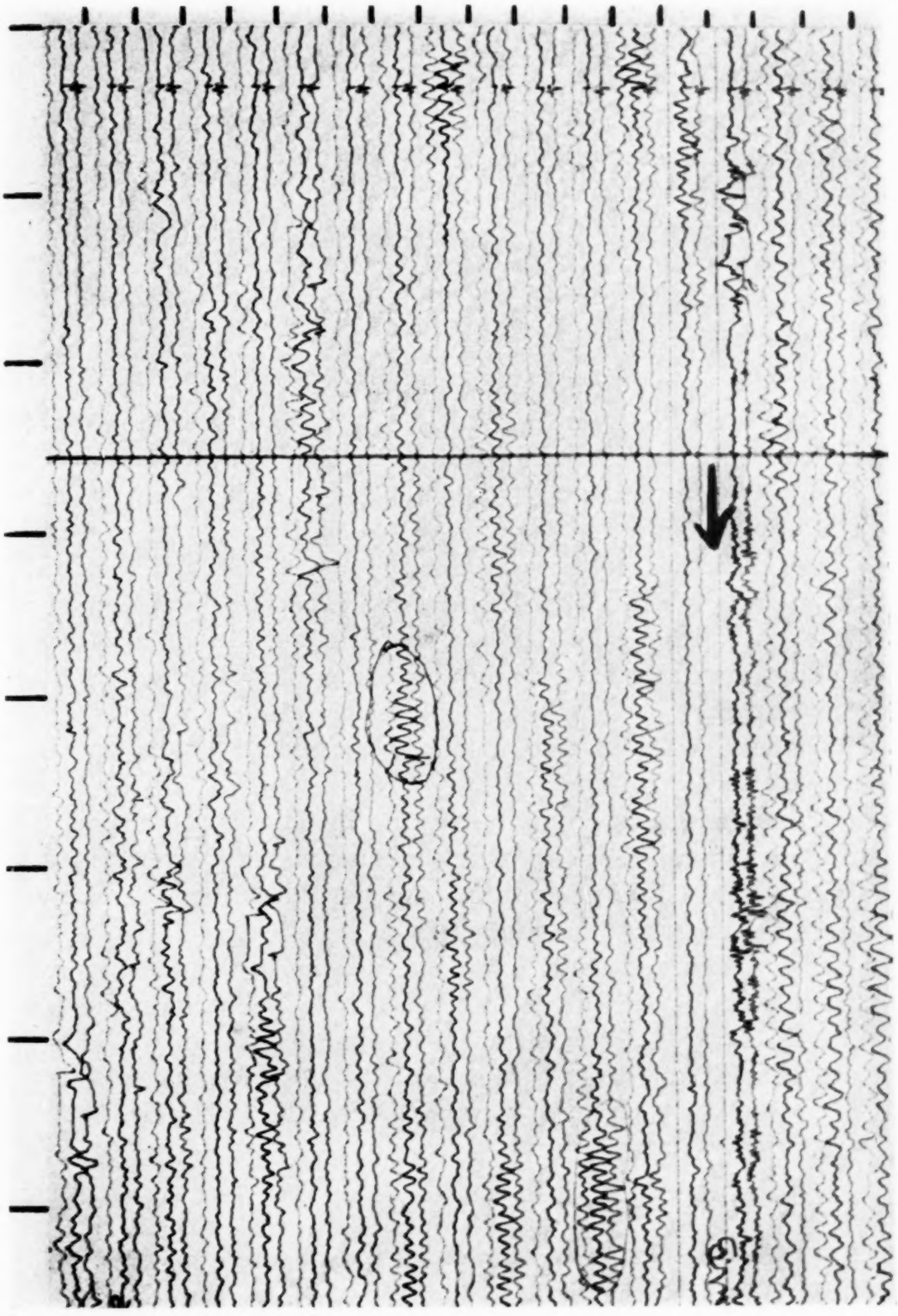

FIG. 6

definitely slower as indicated by the following counts of the rate per second: Before alcohol consumption, 10.62, 10.80; after alcohol consumption, awake, 9.78, 9.96; asleep, 9.74, 9.67. Next day after lunch, 11.25, 11.30, 11.00, 10.95. The marked alpha rhythm only began to disappear when the subject had been deeply asleep for one half hour. Alcoholic stupor like hypnosis does not exhibit the characteristic of sleep.

Infants

Not enough records have been taken to state definitely the changes with age, but certain indications will be presented. Through the kindness of Dr. Myrtle McGraw of the Normal Child Development Clinic, we have studied two babies every two weeks from 17 to 71 days of age as well as some older children. This is the beginning of a definite program of study of the potential rhythms in these babies to be carried out in collaboration with Dr. McGraw every few weeks during the first years of their life. These babies, awake or asleep, have so far showed practically no indication of alpha rhythm, although when awake there is so much muscle potential and movement artefact that a long quiet record could not be obtained.

There was also no alpha rhythm in a baby 126 days old, and in general these records look much alike whether the baby is awake or asleep and are best described as giving random potentials (see Fig. 7*D*).

Two identical twin girls, 27 months old, gave records practically identical in character. The behavior of both was also similar. They seemed afraid of the strange surroundings and when about to fall asleep both gave a peculiar sobbing cry. Little if any alpha waves appeared but when drowsing,

Fig. 7

A and *B*. Records of two identical twins, 27 weeks old. Electrodes on occiput, one tuned to 14 cycles. Note large 'Saw-tooth' potentials (type I).

C. Record from blind man. Electrodes on left and right occiput. Note rapid alpha rhythm.

D. Record from occiput of a baby 126 days old showing random potentials.

Time in seconds between *B* and *C*. Read from top to bottom and from left to right.

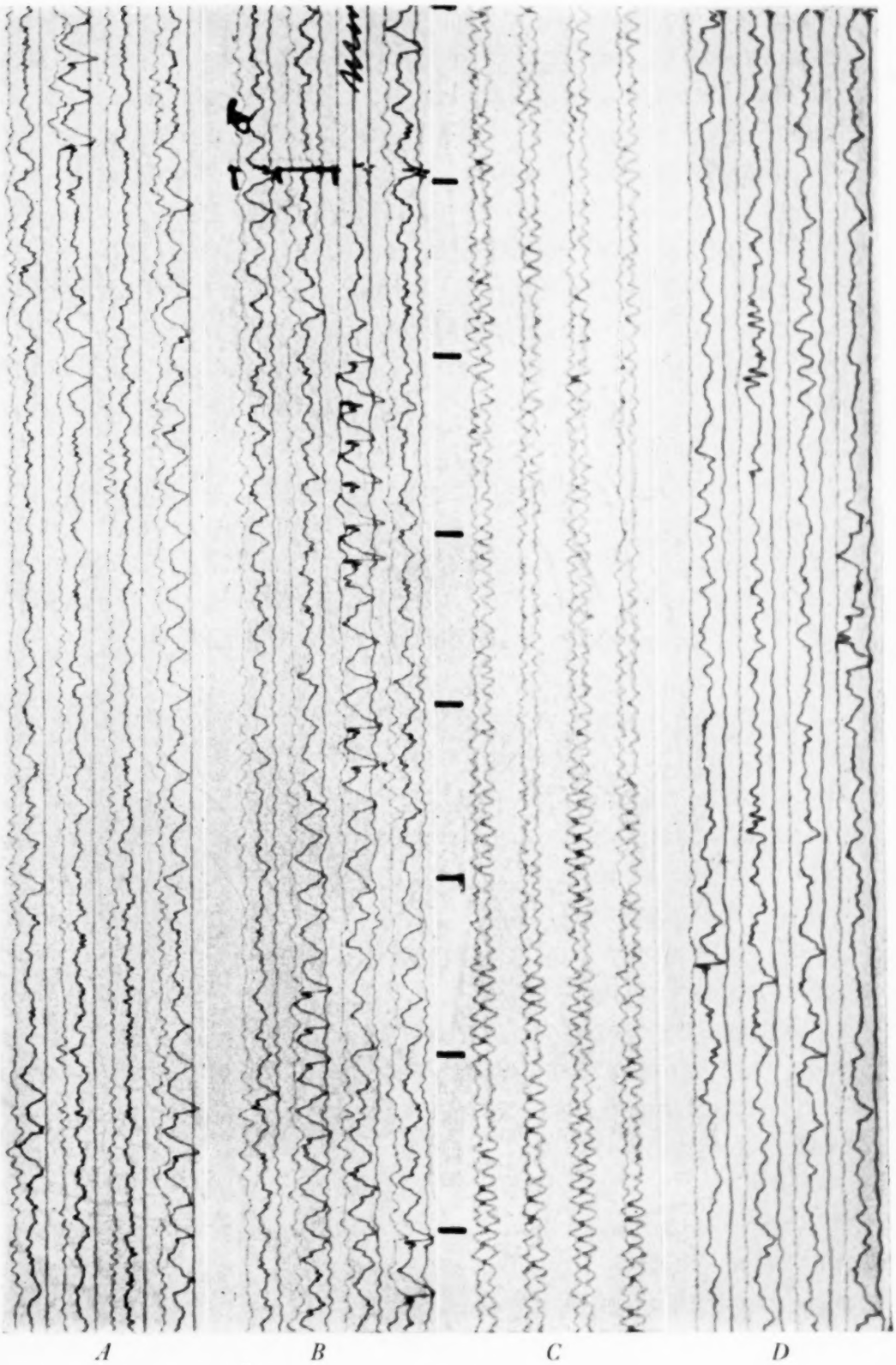

FIG. 7

marked 4 to 5 per second 'sawtooth' potentials (Type 1) of 100 microvolts amplitude were apparent (Fig. 7*A* and *B*). In deep sleep the potentials might be described as irregular with some high frequency. When disturbed during sleep by speaking or touching gently with cotton the sawtooth potentials returned. Some spindles (Type III) picked up by a tuned amplifier appeared during sleep.

A third girl from a different family (R. N.), 28 months old, said to be more intelligent and certainly not as fearful of the experiment, gave random potentials when asleep, and when awake a regular, 7 to 8 per second rhythm. These appeared at times even when the eyes were open but it is not certain that the baby was looking at anything. They may be abolished by having the child look at a watch and return again when a piece of black paper is substituted for the watch. Even when partly awake no sawtooth potentials appeared. It would appear that a slow alpha rhythm affected by visual impressions is present at this age.

Two older twins 3 years 8 months old, not identical, were recorded together. They lay on the same bed with a common ground lead on the crown of their heads and the no. 2 amplifier connected to occiput of one, the no. 3 amplifier to the occiput of the other. The no. 1 amplifier was tuned to 8 cycles and connected alternately to one or the other. The records were quite similar asleep and awake, but one twin gave more alpha rhythms of 8 to 8.5 cycles per second.

A child, 7 years old, showed typical alpha rhythms of 8 cycles per second. In general we gain the impression that lower frequencies are characteristic of young children.

The Alpha Waves

Many experiments dealing with the alpha waves (Type II) have been carried out on one subject (J. D.) and we can largely confirm the findings of Berger, Adrian and others. In addition to sleep the conditions that abolish this rhythm may be grouped as: (1) the attempt to perceive objects, (2) emotional disturbances, (3) concentrated attention.

The Attempt to Perceive

There is no possible doubt but that the attempt to see abolishes the alpha waves, even though there is no emotional reaction on the part of the subject. All observers agree that opening the eyes in a lighted room is the surest method of stopping them and closing the eyes the surest way to start them (see Fig. 8*A*). Opening one eye will stop the alpha waves from both sides of the occiput. Adrian has emphasized the importance of pattern vision as an inhibiting factor, while recognizing that light itself is not a primary stimulus to inhibition. We believe that the part played by vision can best be described as the 'attempt to see' and that this is a more inclusive statement of the factors concerned than pattern vision.

Specific experiments, which confirm Adrian's observations, will illustrate this point. The subject reading a book showed practically no waves over a ten minute period but they appeared immediately the eyes were closed and remained while the book was read to him. Lying in a lighted room with eyes open but not reading, low amplitude waves appeared 10 percent of the time. When an opal bowl, uniformly illuminated, was placed over his eyes, the waves appeared fairly regularly, *i.e.*, in the light but with no attempt to see, the waves are nearly as continuous as in the dark.

However, with the eyes continuously open in the dark, lighting the bowl uniformly will invariably stop them for a time but they appear again in some seconds. It is not necessary that the eyes be shut and opened simultaneously with the appearance of light, but the stopping of the waves is always more pronounced and lasts longer with movement of the eyelids. Associated with this movement is undoubtedly the attempt to see. The mere appearance of uniform light itself is a similar signal to perceive.

Further experiments in complete darkness illustrate more clearly this point. The subject was told to keep his eyes open continuously but that at the word 'now' he was to imagine he saw a face and at the word 'stop' he was to relax and imagine nothing. After the word 'now' the operator further suggested

that he saw clearly the ears, the shape of the nose, the mouth, etc. The result was perfectly clear and was repeated several times. The waves stopped when attempting to see the face and started again at the word 'stop.'

The above experiment was also modified in the following way. The subject (in complete darkness) was told that he would see a face when, at the word 'open,' he opened his eyes and that he would not see a face when, at the word 'close,' he closed them. When the eyes were opened the waves disappeared, only to return when the eyes were closed again. On the other hand, if, during conversation with the subject, he was told casually to open his eyes (without any attempt to see) the waves continued just as though his eyes were shut. The mere movement of the eyelids has itself no effect on the waves.

In view of our experiments during hypnosis it must be mentioned that this subject had never been hypnotized. Nevertheless he was decidedly amenable to suggestion, but it was not possible to start the waves by telling him he saw nothing when his eyes were open in a lighted room. In some persons the alpha rhythm did not stop when asked to think of a visual image. In another person who could visualize a succession of fleeting images, there was much less alpha rhythm when he attempted to do so.

Conditioning

Another interesting experiment may be described as 'conditioning.' If the subject lies in complete darkness with eyes open, a low tone stimulus lasting 5 seconds will not stop

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FIG. 8

A. Record of M. M., showing effect on alpha rhythm of opening and closing eyes (within rectangle at left). Electrode position *B.* Left line from occiput (tuned 10 cycles), middle line from occiput (untuned), right line from right of occiput (untuned). Note that the alpha rhythm is abolished on opening eyes and returns on closing them. Also that the tuned amplifier responds to an alpha rhythm that may be obscured by superposed potentials in the record from the untuned amplifier.

B. Flicker potentials from N. H. Electrode position *B.* Left line, flicker frequency, middle line from left occiput, right line from right occiput. Note that brain potentials follow flicker frequency or are double flicker frequency.

Time in seconds between *A* and *B*. Lines at top separate revolutions of drum (1/2 min. apart). Read from top to bottom and from left to right.

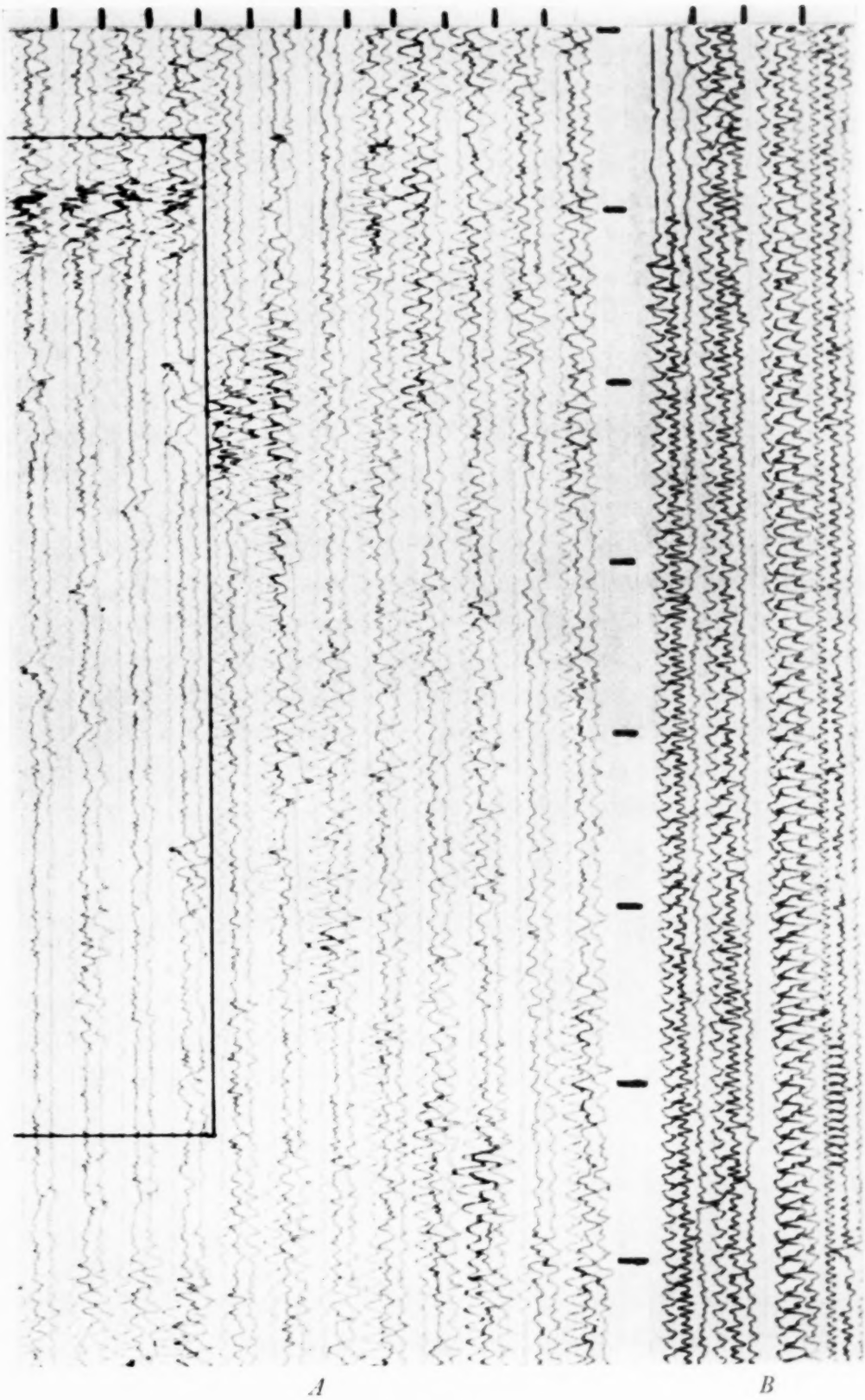

FIG. 8

the alpha waves. If the low tone and also a light stimulus are both presented simultaneously several times in succession at half minute intervals, the waves will of course stop, due to the attempt to see induced by the light, but if now the tone alone is sounded the waves also stop as the attempt to see is now induced by the sound. However, the effect of the tone alone will not last more than two or three times as the conditioning is not permanent and the sound no longer induces the attempt to see. We have observed this effect on several persons and regard it as quite analogous to a conditioned reflex. The tone must not be loud enough to startle the subject, for then the 'startle' effect will complicate interpretation.

Reaction Time

The time necessary to stop the alpha waves was determined by lighting a neon bulb before the eyes of the subject in a dark room. He was also requested to press a key with his hand the moment he saw the light and also when it was turned off. Lighting and darkening of the neon bulb caused an inductive kick in the amplifiers which showed in the record and pressing the key also activated one of the pens so that reaction times to light were automatically recorded. The waves disappeared in 1 or 2 cycles after the neon bulb lighted, *i.e.*, in 0.1 to 0.2 second (average 0.16 sec.), whereas reaction times with the hand operated key were about twice as long (shortest 0.26 sec.; average 0.35).

When the neon light was turned off, 1 to 1.5 seconds elapsed before the waves started again; the shortest time was 0.7 second. This is not surprising, as we found our subjects could postpone the time of reappearance of the waves if they thought of the visual image after it had disappeared. After images are also known to delay the return of the waves.

Light Intensity

Even the faintest light is sufficient to abolish the waves. In one experiment a disc of light 0.3 cm diameter and 30 cm from the subject was made so faint as to be visible only with dark adapted eyes and yet the waves stopped and reappeared

as it was turned on and off. It is to be expected that very faint lights would be fully as effective as bright ones since faint lights would require a greater effort to see them than bright ones.

Flicker

We have repeated Adrian's experiments on flicker using both a movie projector and also a sector disk to interrupt a beam of parallel light falling on the subject's closed eyes or an opal glass bowl over his open eyes. In four persons examined there were three in which at times a perfectly definite potential response to the flicker frequency occurred over the range of 10 to 20 cycles. Frequently the potential response was double the flicker frequency, with the alternate waves sometimes of much lower amplitude (Fig. 8*B*).

In one subject (J. D.), who shows the most regular alpha rhythm, there was no indication whatever of flicker potentials. At most the alpha rhythm was slightly slower when the flicker frequency was slow and slightly faster when the flicker frequency was fast, but this effect was not marked. He was also tested when asleep by directing a bright flickering beam on the closed eyelids but no evidence of flicker potentials appeared.

Emotional States

We have observed, as have others, that many persons when first subjected to tests show few alpha waves during the first hour or so. Later, when they have become accustomed to the electrodes and their strange surroundings, the alpha rhythm usually appears. While emotional conditions are difficult to evaluate we have attributed this lack of waves to embarrassment or apprehension.

Perhaps in the same category may be classed the cessation of rhythm after a startling noise or in the case of a hypnotized person after a pin was thrust through his skin, even though he had been told previously that no pain would be felt and he showed no movement or gave no outcry.

At a given signal one of our subjects (accumstomed to be psychoanalyzed) endeavored to imagine himself in a terrifying

situation (a phantasy of fear). This caused the alpha rhythm, which had been appearing regularly, to cease. This was repeated two more times. On the second trial the waves also stopped but on the third trial they stopped only momentarily. The subject reported that the phantasy was difficult to repeat the third time.

Attention

When a person tries to multiply two figures in the shortest possible time, *i.e.* when he becomes 'rattled' the waves will stop. It is a difficult question to decide whether this effect is emotional or the result of *concentrated* attention. We do know that ordinary concentration of attention, such as listening, reading, reciting, adding simple numbers do not stop the waves.

The Blind

Adrian has suggested that the alpha waves should be absent in a blind person and believed that he had confirmed this conclusion on three subjects that had been blind for some years. Our interpretation of these results is that the blind persons were emotionally disturbed because of the strangeness of the tests.

One of our subjects, (M. C.) 25 years old, had been blind since birth. When he was first tested, practically no alpha waves appeared. Later in the day after lunch when the subject was thoroughly at ease and after he had confessed that he was no longer apprehensive, the alpha waves appeared regularly. After he had been asleep for some time, the waves became random (Type VI) followed by the characteristic 'spindles' (Type III). While asleep, a cough outside the room (which he remembers hearing and which evidently slightly awaked him) again gave rise to continuous alpha waves lasting for six minutes (see Fig. 7*C*). Such behavior is quite characteristic of a person with sight, and in no way have we found the potentials of this blind subject to differ from a normal person. The average frequency of his alpha waves was 12.1 per second. His spindles were 14.7 per second.

Hypnosis

The subject was a ship's carpenter, 44 years old, a person who had been frequently hypnotized and who had been completely cured of a hemilaternal migraine by this method. He was brought to the laboratory by Dr. David Slight of McGill University, who hypnotized him and collaborated in the experiments. Eight records were made, six during hypnosis, each lasting about two hours.

Records without hypnosis, during the latter part of which the subject slept lightly and normally, showed characteristic alpha rhythms, abolished upon opening the eyes at a tone signal and reappearing upon closing the eyes when the tone was discontinued. The tone signal experiments were repeated many times in order to condition the subject to open and close his eyes upon hearing the tone sounded and discontinued. The alpha rhythms became fewer when drowsing and a few spindles appeared. In every way the record was normal.

The subject was then hypnotized in a dimly lighted room and passed into the hypnotic state very quickly. The cataleptic condition of his muscles was marked. No change in character of the alpha rhythm, which continued during hypnosis, could be observed, except a slight slowing, as observed in Table 3. The automatic tone signal used previously while he was unhypnotized and to which he had responded by opening his eyes, was turned on. The subject now responded without any suggestion by opening and closing his eyes. The effect was the same as before hypnosis, whenever his eyes were open the rhythm stopped, and whenever closed the rhythm began again.

It was then suggested to the subject that no tone would be heard but only the hypnotist's voice. On sounding the tone evidently a struggle ensued, as evidenced by the subject's expression and appearance of muscle potential in the record. The first few times the eyes opened, the alpha rhythm stopped, but later there was no opening of the eyes upon sounding the tone and the rhythm continued.

These experiments all indicate that even during hypnosis, if the eyes are opened in a lighted room, the rhythm ceases, but

that it returns again upon closing them just as in a normal person. During all the hypnosis experiments there was no appearance of random or 14-cycle potentials, characteristic of normal sleep and it would seem that the term hypnotic 'sleep' is not a correct one for the hypnotic state, at least as judged by the criterion of brain potentials. It is interesting to note that Whitehorn, Lundholm, Fox and Benedict find that the metabolic rate during hypnosis is not characteristic of sleep and likewise conclude that 'hypnotic sleep' is an incorrect term.

The next experiment was to determine if, upon opening the eyes in a lighted room, it was possible to suggest that he saw nothing, *i.e.*, to induce temporary blindness, which would be indicated by the appearance of the alpha rhythm. As we soon found that the act of opening the eyes itself suppressed the rhythm and that it was not possible to easily overcome this by suggestion, the subject's eyelids were permanently fastened open with adhesive tape. Now when the suggestion was made that he saw nothing, the alpha rhythm appeared but when it was suggested that he saw a spot on the ceiling (actually there) the rhythm disappeared, only to appear again when told he saw nothing. This was repeated 16 times with invariable results.

This experiment shows that temporary blindness as indicated by the appearance of the alpha rhythm *can* be induced, provided the powerful tendency to attempt to see associated with opening of the eyelids is removed by keeping the eyelids permanently open.

Further experiments carried out with eyes taped open indicated very definitely that it was possible by suggestion to cause the waves to appear and disappear. This was true whether the room was completely dark or whether a light was shining directly in the subject's eyes. It was noticed, however, in the latter case that much muscle potential was present in the record. The subject evidently found it more difficult to follow the suggestion when the light was on. Nevertheless the brain response followed the suggestion whether the light was dim or bright. These responses were immediate on suggesting that the subject could see but delayed four or five seconds on the suggestion that he was blind.

Other experiments were carried out during hypnosis (eyes closed in a dimly lighted room) to determine the effect of stimuli on the subject. The subject was *threatened* with a pin prick several times but no change in the record appeared even when the suggestion was made that the pin was in the arm and painful. A short time later, after repeated suggestions that the subject would feel nothing, the pin was actually pushed through a fold of skin of the forearm. The waves stopped at this moment and remained absent for 28 seconds; then they began again. After 63 seconds the pin was removed and no change in the record occurred. Suggestions that the subject would feel nothing were continued and the pin thrust through the skin in two more trials. There appeared to be some inhibition of the rhythm on insertion of the pin but no change on removal. However, the results of the second and third trials were not as clear-cut as on first trial. It is possible that the repeated suggestion abolished the 'startle' or shock effect which one might expect as the result of potentially painful stimuli. When the pin was inserted the subject made no outcry or movement and from all appearances behaved as if nothing had happened. No muscle potential appeared in the record.

High pitched sounds or disagreeable noises did not change the record.

The subject during hypnosis could add figures mentally without any change in the record except that muscle potential appeared when the sum was incorrect or could not be done.

Summary

A convenient arrangement of apparatus for studying brain potentials is described. This includes 3 amplifiers for three separate recordings from the scalp. The records are made on paper on an eight foot drum so that eight hours of continuous recording can be taken and easily inspected at any future time.

Variation in the records of different individuals is described and the types of waves classified. Adults differ in the frequency of their waves, in the amount of disturbance which abolishes these waves, in the extra potentials superposed on the

regular rhythms and in their response to various stimuli and emotional states.

Identical twins give records identical in general character.

Whenever a subject lies quietly for two hours or more, the frequency of the alpha waves is from 5 percent to 10 percent slower at the end of the period than at the beginning.

Very young babies have no regular rhythms; children tend to show lower frequencies.

When drowsing the rhythm becomes less and less frequent and in sleep the record changes to random potentials with an occasional short 14 cycle burst of waves, most marked from the top region of the head. Sounds or other disturbances during sleep even though not awakening the sleeper bring back the alpha rhythm, which appears to be connected with a change in the general level of brain activity.

Mental activity as such does not seem to affect the alpha rhythm.

Emotional states do have a profound effect upon the waves. If a subject is embarrassed or apprehensive practically no waves appear.

The appearance of light stops the waves in 0.1 to 0.2 second and lights near the threshold of vision are effective. The influence of light is not a direct effect but rather a secondary one and can best be described as the 'attempt to see.' Opening the eyes in a dark room will stop the alpha waves if the subject expects to see something but otherwise has no effect.

A subject can be conditioned to a light and a low tone signal presented simultaneously several times in succession so that after a time the tone signal alone will stop the alpha rhythm although a tone ordinarily has no such effect.

Flickering lights give rise to potentials in some persons that exactly follow the flicker frequency over a limited range.

A blind person gave a normal record of potential with alpha rhythm present but only after embarrassment and apprehension of the strange surroundings had passed.

During hypnosis the alpha rhythm is slightly slower than normal but otherwise the record is no different from a normal person awake. There was no indication that this condition is

similar to sleep. A pin thrust through the skin stopped the alpha rhythm although there was no movement and no evidence that pain was felt. By suggestion that the subject could see or could not see, the alpha rhythm could be stopped or started again. This was true whether the room was light or completely dark.

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THE LOUDNESS OF COMPLEX SOUNDS*

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The purpose of this experiment was to study the variation in the loudness of certain complex sounds resulting from changes in the intensities and frequencies of the components.

Those investigators who first studied loudness quantitatively thought that it varied solely with the intensity of the physical stimulus. Actually, no such simple relationship between loudness and sound intensity exists. The loudness of an auditory stimulus is a complex function of the frequency, physical intensity, and in some cases, the phase relations of the stimulus (**3**, **10**). Individual differences among the hearers as well as the conditions under which sounds are heard are also important factors in loudness (**12**). The truth of these statements has emerged slowly from experiments performed since the development of electrical methods for the production and accurate control of sound stimuli.

Direct comparisons of the loudness of pure tones have shown that two different frequencies may be of the same physical intensity and still be judged of different loudness. Now, since the value of the minimum audible intensity changes with frequency, it might be supposed that if the threshold were used as a base line of equal sensation, *i.e.* if two tones were increased in intensity by the same amount above their respective threshold values, they might sound equally loud. This is not the case (**9**). However, it has seemed possible that a modification according to Fechner's law might be true. The law (S equals k log R) presupposes that Weber's law is correct. Early determinations of the differential intensity threshold for audition (made with accurate electrical instuments) gave values of the Weber fraction $\Delta R/R$ which were sufficiently alike to make Weber's

* From the Psychological Laboratory of Princeton University.

law tenable (**16**, **19**). The attempt to express loudness as decibels above threshold was based on such results. More recent experimentation has extended the intensity and frequency range used (**13**, **16**, **18**, **20**). The data show that the value of Weber's fraction is fairly constant for the middle range of frequencies at the moderate and high intensities, but changes markedly at the low (32–256 cycles) and at the high frequencies (above 4000 cycles). The fraction also shows a rapid decrease from the threshold to an intensity approximately 30–40 decibels above this value.

Since Weber's fraction is constant only within a restricted range of frequencies and intensities, Fechner's law could only apply to that range. Those who used decibels above threshold as a scale of loudness soon encountered this limitation when they discovered that a low tone and a tone of the middle range might be an equal number of decibels above threshold and still be very different in loudness (**1**).

Fechner's law assumes not only that all j.n.d.'s are equal to a constant ratio of the stimulus, but also that they are equal units of sensation and that they can be integrated. Hence two tones of different pitch, but an equal number of j.n.d.'s above threshold should be equally loud. The discovery that this inference was probably incorrect (**8**) proved to be the coup de grace to the notion that Fechner's law expressed the relation between the stimulus and the sensation of loudness.

The value of $\Delta R/R$ is not constant with changes of frequency and intensity, and, psychologically, it does not represent equal increments of sensation over the whole range of audible sounds. Consequently, it has failed to be of use as a unit in the construction of a loudness scale.

When these facts became known, investigators cast about for some other way of constructing a scale of loudness.

The variation of loudness with pitch might be taken into account if the loudness of all tones was expressed in terms of an equally loud reference tone. That is to say, the loudness of different frequencies over the range of audible intensities might be compared with a standard tone by a group of persons

of normal hearing. This procedure would give the different physical intensity values of a reference tone which seemed as loud as the various comparison frequencies and intensities used. Empirical observations of the sort outlined were collected by Kingsbury (**15**). The standard frequency was set at a given intensity value and then compared with some other frequency whose intensity was varied until the subject judged the two tones to be of equivalent loudness. This procedure was repeated for a number of comparison frequencies between 50 and 4000 cycles; also for a number of intensity levels of the standard frequency.[1] Twenty-two subjects were used.

Kingsbury expressed his results in the form of a set of curves called equal loudness contours. In Fig. 1 (taken from Fletcher and Munson (**10**) as typical of such curves), the abscissa represents the frequency, the ordinate the intensity of the comparison tone. Each contour shows that intensity value of the reference frequency which is equal in loudness to all other tones represented by points on the contour. For example, the contour marked 30 shows the various intensities for tones between 25 and 20,000 cycles which are judged equal in loudness to the reference frequency at 30 decibels above threshold. In the figure here shown the reference frequency is 1000 cycles.

The validity of the reference-tone method of expressing loudness rests on the assumption that all tones represented by points on the same contour are not only equal in loudness to the standard frequency, but also are equally loud when compared with one another. Kingsbury found experimentally that this assumption was true, and his results have recently been confirmed by Churcher, King and Davies (**7**).

Since Kingsbury's work, the range of frequencies and intensities involved has been extended and the experimental technique improved in various ways by Fletcher and Munson

[1] The variability of judgment in this and similar work to be described subsequently is large. For this reason, all statements concerning loudness must be understood generally to refer to the average of repeated judgments by a group of subjects. This is the meaning of the term 'average normal ear' and the results of any one subject may depart by several decibels from it.

(10). Their data show fair agreement with those of Kingsbury when certain differences in experimental procedure are taken into account. (*Cf.* 10, Appendix B.)

In the work of Fletcher and Munson the reference tone was chosen as 1000 cycles. This is a practical reference frequency because it lies in that region of the whole range of auditory stimuli to which the ear is most sensitive and covers a wide range of loudness from its minimum audible value to its threshold of feeling.

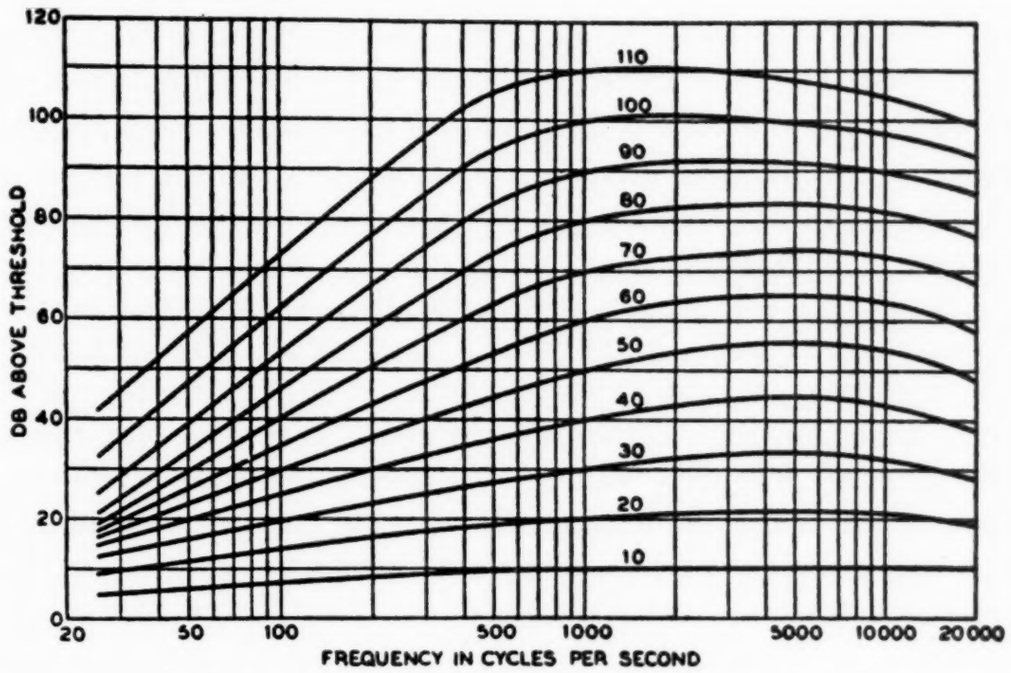


FIG. 1. (Taken from Fletcher and Munson (10), page 17.) Each contour shows that intensity value of a 1000 cycle tone which is equal in loudness to all other tones represented by points on the contour.

The intensity of the reference tone is measured in decibels above a fixed intensity level (10^{-16} watts per square centimeter) rather than in decibels above threshold. If one knows the intensity of any sound 'x' whose loudness is unknown, one can express this loudness in terms of the 1000 cycle tone by using the equal loudness contours. That intensity of the reference tone which lies on the same contour with the intensity of the unknown sound 'x,' is of the same loudness as 'x.' The intensity value of the reference tone is called the loudness level of sound 'x.' This definition of the

term *loudness level* should be kept clearly in mind and distinguished from *loudness*, which is the term used to express the magnitude of an auditory sensation.

Such a method of determining loudness leaves much to be desired, since the loudness of all frequencies is stated in terms of the intensity of an equally loud reference tone, and this intensity is expressed either in decibels above an 'average normal threshold' or, as in this case, in decibels above some reference intensity. The scheme would be satisfactory if the loudness of the 1000 cycle tone were a strict logarithmic function of its intensity, *i.e.* if the Weber-Fechner law truly expressed the functional relation between stimulus and sensation for the 1000 cycle reference tone, even though it did not apply at other frequencies. Then for geometric increases in intensity the reference tone would show arithmetic increases in loudness.

However, this is not the case (**8**). Consequently, the loudness level scale, employing a 1000 cycle reference tone, has meaning only to persons who by frequent hearing of a 1000 cycle tone have come to know what, in terms of loudness, the different intensity values of a 1000 cycle tone represent. In order to make the loudness level scale have more meaning for the average person, different intensity values of the reference tone are often marked with words indicating the loudness in terms of a commonly experienced situation *e.g.* subway noise, average speech and so on (**1**). At best this is a very crude expedient.

Since Fechner's law does not express the relation between stimulus and sensation exactly, attempts have been made to arrive at some function which would do so, particularly for the reference tone of 1000 cycles. Three principal modes of approach can be distinguished.

In one approach, which is a variant of the method of supra-liminal sense distances, the relation between stimulus and sensation is determined from estimates of the relative loudness of tones of different intensity (**7, 12, 14, 17, 22**). A sound of a given intensity is presented to the listener, who compares it with the same frequency at a different intensity.

He signals the experimenter when the loudness of the comparison tone appears to be reduced (or increased) a half, a quarter, or some other fraction of the original loudness. The function relating loudness estimates to changes of intensity and frequency can be determined by using different frequencies and different initial intensity levels. Then, choosing arbitrarily certain intensity values as the limits of the scale (for example, the average minimum audible value as the lower limit and 100 decibels above the 800 cycle threshold as the upper limit) one can construct empirically an arithmetic scale of loudness on the basis of the data. This has been done by Ham and Parkinson (**14**) and by Churcher, King and Davies (**7**). The numbers on such loudness scales are related to each other in such a way that a sound twice as loud or half as loud as another sound will have a number approximately twice or half as large. There is some disagreement among the experimental results of the several groups of men who have accumulated the basic data. Reasons for some of the differences have been suggested by Geiger and Firestone (**12**).

Another approach to the problem has been made by Davies (**8**), who confined his attention to the relation between stimulus and sensation for the reference tone alone. The loudness of all other tones, according to his idea, would be stated in terms of the reference tone, and hence only its stimulus-sensation function is important. He assumed (*a*) that loudness estimates are based on the total number of nerve impulses reaching the cerebrum every second, and hence that a tone heard binaurally would be twice as loud as when heard monaurally. He further supposed (*b*) "that the value of the minimum perceptible change of intensity depends upon the magnitude of the sensation, and hence, as a corollary, that when different methods of applying different stimulus intensities result in the same value of minimum perceptible change, the sensation magnitudes are the same" (**8**, p. 943). It is known from experimental data on the monaural and the binaural differential intensity thresholds (**6**) that the binaural is smaller than the monaural differential threshold for any intensity level from threshold to 90 decibels above threshold.

(See Fig. 2.) On the basis of the foregoing assumptions Davies used these data to determine the relation between stimulus and sensation for an 800 cycle tone. Curve *C* shows the value, in decibels, of the minimum perceptible change of intensity for any intensity level of the 800 cycle tone when heard with one ear. Curve *D* gives similar values for binaural listening. According to assumption (*b*) above, when the value of the minimum perceptible change of intensity is

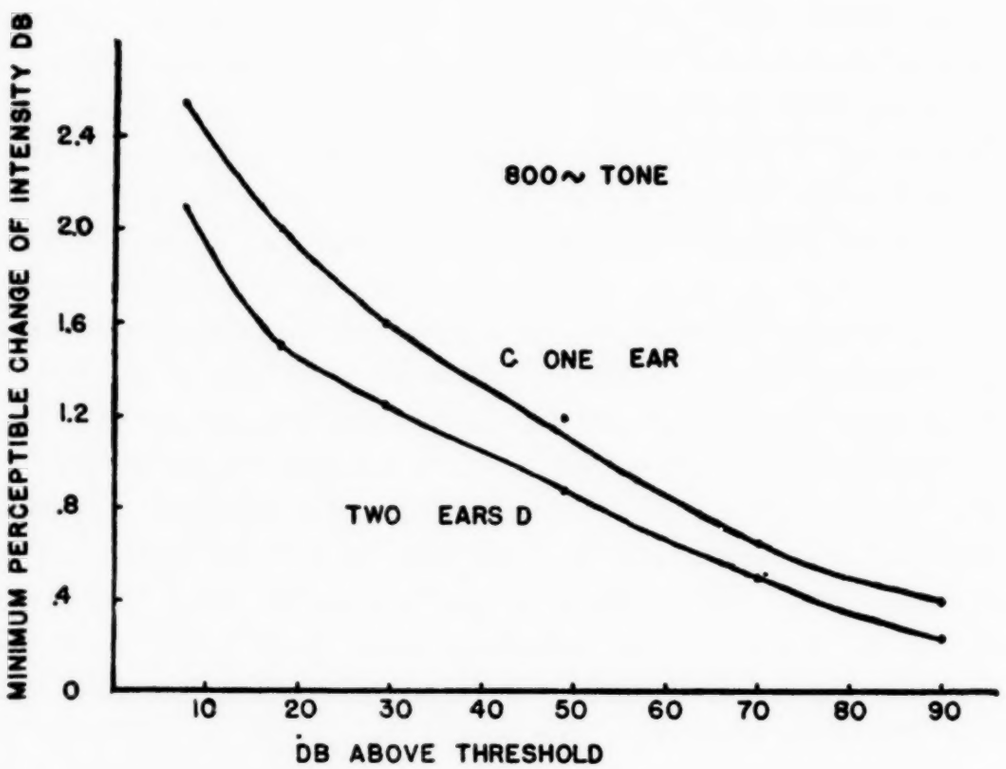


FIG. 2. (After Churcher, King and Davies (6), p. 938.) Minimum perceptible changes of intensity of an 800 cycle tone.

constant, sensation is of constant magnitude. Hence the two intensities (one for monaural, one for binaural listening) at any particular value of the ordinate should represent two 800 cycle tones of the same loudness. Since the monaural intensity value would seem twice as loud if heard binaurally, the difference between the monaural and binaural intensity values gives the intensity difference necessary for the one tone to sound twice as loud as the other when both are heard by two ears. For example when the value of the minimum

perceptible change of intensity is 1.6 decibels, the 800 cycle tone is 16 decibels above threshold when heard by two ears, and 29 decibels above threshold when heard by one ear. The one ear value (29 decibels) when heard by two ears should, according to assumption (*a*) be twice as loud as the 16 decibel value. The intensity difference shows the amount by which an 800 cycle tone, at 16 decibels above threshold, must be increased to appear twice as loud.

Davies determined the function relating stimulus to sensation from the empirical values in the way just described. He asserts that, when plotted, this function is in close agreement with that derived from the data on fractional loudness estimates previously described. The theoretical curve which this method gives for the loudness of sounds heard by one ear as compared to the loudness of those sounds when heard by two ears, is in good agreement with the empirically determined curve of Fletcher and Munson to be considered later.

The two methods just discussed were employed in attempts to measure or, more correctly, to make a scale for the loudness of pure tones. Such an effort in respect to complex sounds is a much more difficult task.

Early efforts to calculate the loudness of a complex sound were made by Fletcher and Steinberg (**11**), Steinberg (**23**), and Barkhausen and Lewicki (**2**), but the formulas which they developed were found to be inadequate.

The following method for determining the loudness level of a complex sound, when its component frequencies and intensities are known, has been used by Churcher, King and Davies (**5**). The component frequencies of the sound are determined by an accurate sound analyzer which gives acoustical pressures in absolute units. With the aid of equal loudness contours these components are then expressed as intensity values of an equally loud 800 cycle reference frequency. These intensity values are summed and expressed as decibels above the 800 cycle threshold, to indicate the loudness level of the noise. The effects of masking are not taken into account. Such a method of calculation has yielded fair agreement with the results of loudness balances between

noises and an 800 cycle tone except in the case where the components of the noise are harmonics, as in transformer hum. Churcher, King and Davies conclude that in such cases loudness can not be calculated, but may be determined only by direct comparison with an 800 cycle reference tone.

The most recent attempt to state the relation between the physical characteristics of a complex auditory stimulus and its loudness as judged by the average normal ear is that of Fletcher and Munson (**10**). They have developed an empirical formula for calculating the loudness level of any steady complex sound. In the development of this formula they have derived a function which relates loudness to loudness level in such a way that the loudness contribution of any constituent of a complex sound can be expressed in a simple arithmetical manner. In other words, sounds of different pitch which are judged to be of equivalent loudness have equal numbers, and a sound half as loud as another has a number of half as large. To obtain the function, Fletcher and Munson employed the following procedure. A group of subjects made loudness balances between a 1000 cycle reference tone and complex sounds of two components or ten components. In some cases the frequencies and intensities of the components were selected so that interference did not occur. They also obtained data showing how much the intensity of a tone (125 or 1000 or 4000 cycles) heard monaurally had to be increased to sound as loud as the same frequency heard binaurally. The data from complex tones and from one vs two ear hearing were then used to derive empirically a set of numbers expressing the functional relation between loudness and loudness level.

Fletcher and Munson have also devised a method for combining the loudness numbers representing the components of a complex sound which takes into account masking effects in the complex. The factor which takes account of masking was derived from data on masking at the threshold obtained by Wegel and Lane (**24**), and from the results of loudness balances with complex sounds.

The principal assumptions involved in the derivation of the masking factor are three in number:

1. The entire diminution in the contribution of any component to the total loudness as a result of masking may be attributed to that neighboring component of lower pitch which causes the greatest masking. On this basis the fundamental always contributes all of its loudness value to the total.

2. Within limits, the interference increases as the components are brought closer together in frequency.

3. When two components are almost identical in frequency they may be treated as a single frequency, the exact value of which is determined by a weighted frequency value and a summation of intensities.

The theoretical results obtained by means of the Fletcher and Munson formula are in fair agreement with the empirical results obtained by Ham and Parkinson and by Geiger and Firestone for loudness estimates (**12**, **14**).

E. J. Abbott has found that the formula gives results for noises comparable to the values obtained by direct comparison with a reference tone (**1**).

Churcher (**4**) has objected to the formula because it fails to take into account the peculiar characteristics of a complex auditory stimulus whose components are harmonically related. Churcher, King and Davies have found that in a sound of this sort, "the removal of components which did not appreciably contribute to the total energy produced a change of 12 db" in the intensity of the equally loud reference tone (**5**, p. 224). Churcher maintains that since the Fletcher and Munson formula fails to take such an effect into account, it cannot yield correct loudness values in many cases (**4**).

The present experiment was designed as a further test of the Fletcher and Munson formula and of the general concept of determining the loudness of complex sounds.

Apparatus and Procedure

The subject was presented alternately with a single pure tone and with a complex sound which consisted of two pure tones produced simultaneously. He adjusted the intensity of the single tone until it appeared as loud as the complex sound. Thus the loudness of the complex sound was obtained in terms of the intensity of a single frequency. The tones were produced electrically by two oscillators supplying two telephone receivers as indicated in Fig. 3. The phones, at a ninety degree angle to each other, were placed seven inches from the right ear. The comparison frequency,

generated by Circuit 1 and controlled by Attenuator 3, lasted for one second. The complex sound, composed of tones generated by Circuits 1 and 2 and controlled in intensity by Attenuators 1 and 2, followed a quarter of a second later and also lasted for one second. The duration of the sounds was such as to avoid fatigue effects. The alternation rate is in the optimal range for the judgments required.

The alternation of complex and comparison sounds continued until the subject had adjusted Attenuator 3 so that the two sounds seemed equally loud. Sometimes the comparison tone, sometimes the complex sound, came first. The subject's absolute threshold for each of the frequencies involved was determined at the beginning of each experimental hour, and the intensities of all the tones used in that hour were measured in terms of decibels above their thresholds. It was possible to minimize the tendency of the subject to form a position habit in his manipulation of the attenuator control by changing, from one experimental hour to another, the input and hence the position of the attenuator control for any given loudness.

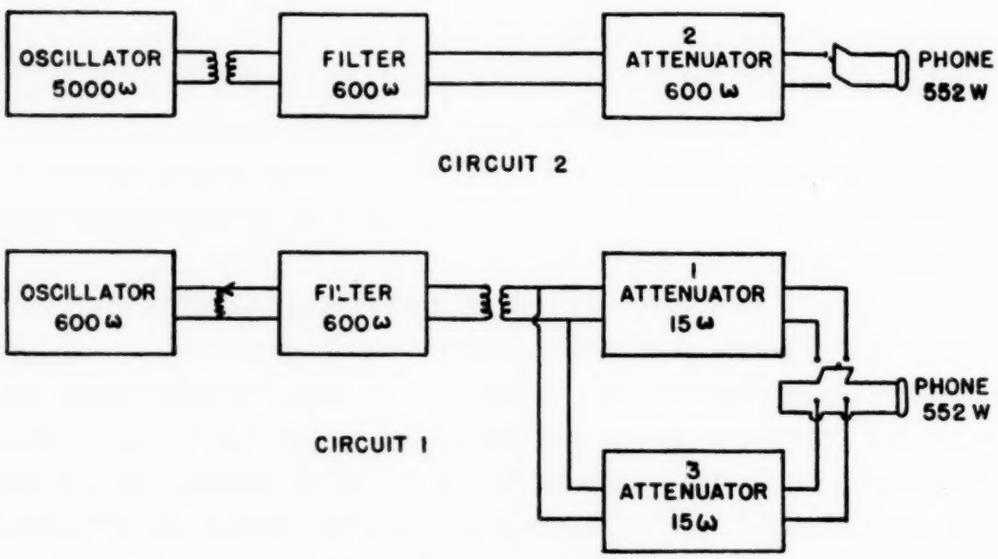


FIG. 3. Diagram of apparatus

Each of three subjects, having normal hearing, made ten comparison judgments (method of average error) on each of the complex sounds. The frequency combinations forming the complex sounds were 1600–1400, 1600–2000 and 1600–4000 cycles. Each of these three combinations was presented in the sixteen different intensity combinations shown in Table 1, so that forty-eight different complex sounds were used. A comparison tone of 1600 cycles was used with all the complexes. In addition a comparison tone of 4000 cycles was employed with the 1600–4000 cycle complexes.

RESULTS

The results are shown in Tables 1, 2, 3, and 4. The first column in each table shows the frequency composition of the complex sound, the second column, the intensity of each of its components, in decibels above threshold. Column 3 shows the average intensity, in decibels above threshold, of

TABLE 1

RESULTS

Freq.	Complex Sound	Comparison Sound S1 1600		Comparison Sound S2 1600		Comparison Sound S3 1600	
	Db Above Thresh-old	Db Above Thresh-old	Range of Judg-ments	Db Above Thresh-old	Range of Judg-ments	Db Above Thresh-old	Range of Judg-ments
1600 1400	10 10	15.8	11–21	14.2	12–18	17.3	14–22
1600 1400	10 20	22.5	19–27	19.7	15–28	24.8	19–31
1600 1400	10 30	29.4	27–36	25.6	22–31	31.9	27–37
1600 1400	20 10	23	22–26	20.9	19–23	23.4	21–26
1600 1400	20 20	26.5	23–31	23.7	19–29	28.1	26–32
1600 1400	20 30	32.4	28–37	27.7	24–32	32.8	28–36
1600 1400	20 40	38.1	34–43	32.4	26–39	38.8	35–44
1600 1400	30 10	31.5	30–33	30	27–33	32.1	31–33
1600 1400	30 20	33.9	33–35	29.7	26–35	32.9	24–37
1600 1400	30 30	36.2	34–39	30.7	29–41	37.1	34–41
1600 1400	30 40	40.4	37–44	36.2		41.4	36–45
1600 1400	30 50	46.5	44–49		35–42	47.5	44–50
1600 1400	40 20	41.3	41–42	38.7	37–41	40.9	34–43
1600 1400	40 30	41.9	40–43	38.9	38–43	42.4	41–45
1600 1400	40 40	44.3	42–46	41.1		45.4	43–48
1600 1400	40 50	50.4	47–52				

TABLE 2

RESULTS

Freq.	Complex Sound	Comparison Sound S1 1600		Comparison Sound S2 1600		Comparison Sound S3 1600	
	Db Above Threshold	Db Above Threshold	Range of Judgments	Db Above Threshold	Range of Judgments	Db Above Threshold	Range of Judgments
1600 2000	10 10	12	9–20	15.6	11–21	15	13–17
1600 2000	10 20	22	16–33	20.7	16–28	20.5	16–24
1600 2000	10 30	27.6	21–31	25.7	20–30	22.3	18–26
1600 2000	20 10	21	17–24	20.8	18–25	22.6	20–25
1600 2000	20 20	25.1	21–28	25.7	22–30	24.4	21–30
1600 2000	20 30	29	22–31	27.9	23–33	26.7	23–31
1600 2000	20 40	35.7	29–43	34.4	30–40	29.7	25–32
1600 2000	30 10	28.6	21–32	28.3	26–30	29.9	29–31
1600 2000	30 20	29	22–32	29.7	28–30	29.9	28–33
1600 2000	30 30	32	28–36	33.3	29–38	31.6	30–36
1600 2000	30 40	37	32–44	37.3	35–41	33.6	29–40
1600 2000	30 50	43.6	36–50			37.6	32–45
1600 2000	40 20	37.1	29–40	39	37–47	39.5	37–41
1600 2000	40 30	37	33–39	37.8	36–39	38.3	37–40
1600 2000	40 40	37.8	32–41	41	37–45	39.4	37–41
1600 2000	40 50					44.2	41–47

TABLE 3

RESULTS

Freq.	Complex Sound	Comparison Sound S1 1600		Comparison Sound S2 1600		Comparison Sound S3 1600	
	Db Above Threshold	Db Above Threshold	Range of Judgments	Db Above Threshold	Range of Judgments	Db Above Threshold	Range of Judgments
1600 4000	10 10	13.7	11–16	13	9–19	16.6	15–19
1600 4000	10 20	16.2	13–21	18.6	12–24	21.2	17–24
1600 4000	10 30	21.3	17–24	22.3	15–26	24.9	21–28
1600 4000	20 10	22.1	20–24	19.7	17–22	23.3	21–25
1600 4000	20 20	24.1	21–27	23.4	18–27	25.6	23–29
1600 4000	20 30	24.6	22–29	26.9	24–32	27.1	23–30
1600 4000	20 40	27.8	22–32	31.1	24–38	30.8	26–35
1600 4000	30 10	31.2	30–33	30.4	27–32	31.1	30–33
1600 4000	30 20	31.9	29–35	30.6	29–32	32.3	30–34
1600 4000	30 30	33.2	30–36	32.3	29–36	33.6	31–36
1600 4000	30 40	34.7	31–39	36	32–42	35.7	31–43
1600 4000	30 50	36.5	32–42	40.1	34–47	38.2	33–43
1600 4000	40 20	41.2	40–42	40.1	39–42	41.4	40–43
1600 4000	40 30	41.9	40–44	40.2	36–42	41.7	40–43
1600 4000	40 40	42.2	39–46	41.5	40–43	42.9	41–44
1600 4000	40 50	43.8	40–48	43.5	40–47	44.3	40–46

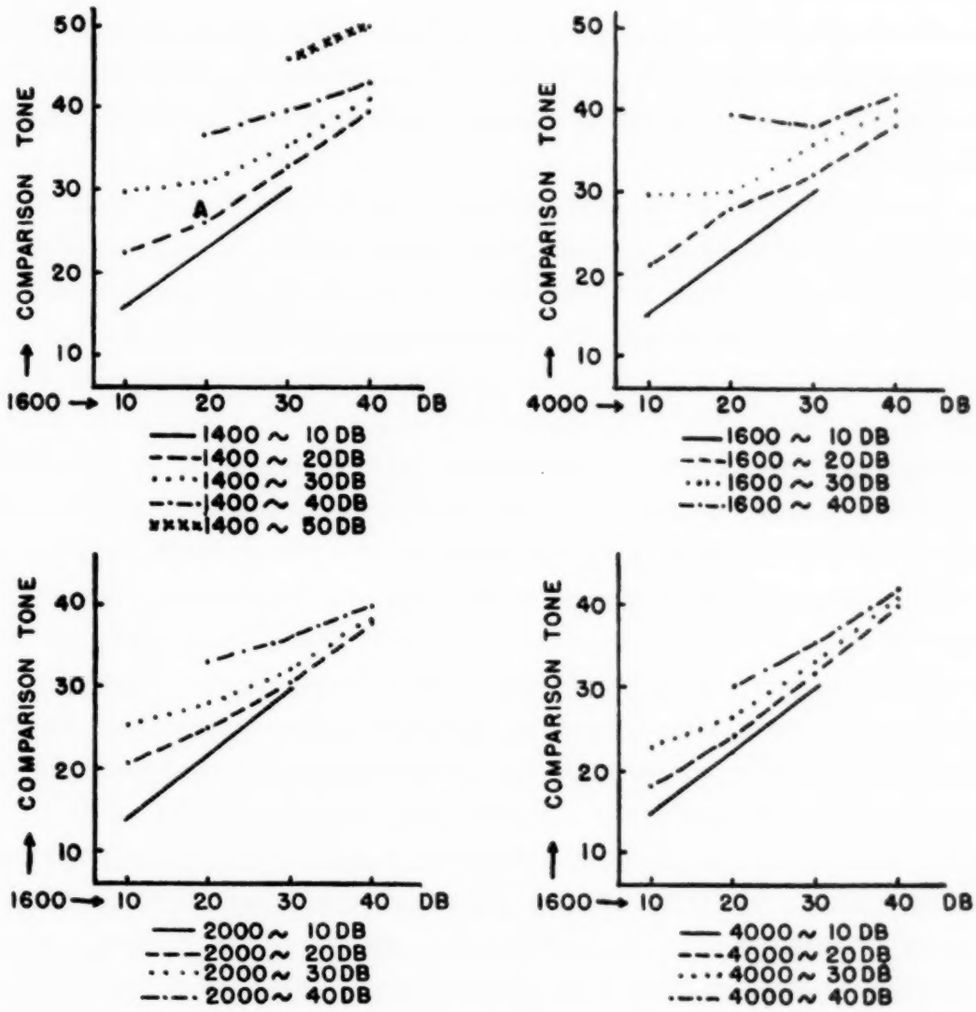
TABLE 4

RESULTS

Freq.	Complex Sound	Comparison Sound S2 4000	
	Db Above Threshold	Db Above Threshold	Range of Judgments
1600 4000	10 10	15.5	13–21
1600 4000	10 20	22.6	19–25
1600 4000	10 30	30.5	28–34
1600 4000	20 10	21.3	15–26
1600 4000	20 20	27.6	21–31
1600 4000	20 30	32	28–36
1600 4000	20 40	38.1	35–41
1600 4000	30 10	29.5	25–32
1600 4000	30 20	30.3	24–36
1600 4000	30 30	36	31–39
1600 4000	30 40	40.3	39–43
1600 4000	40 20	39.5	36–42
1600 4000	40 30	38.2	34–42
1600 4000	40 40	42.3	40–44

the 1600 cycle comparison tone which was judged by subject S1 to be as loud as the complex sound of the frequency and intensity composition indicated in the first two columns. Column 4 shows the range over which are distributed the ten judgments from which the average intensity was computed. Columns 5, 6 and 7, 8 give information similar to that in

columns 3, 4 for the other subjects S2 and S3. Table 4 is similar to Table 3 except that the data are confined to subject S2 and are the results when a 4000 cycle comparison tone was substituted for the 1600 cycle comparison tone. Circumstances prevented the use of more than one subject in the latter procedure.



FIGS. 4, 5, 6, 7. The figures show the diminishing loudness contribution of any constant sound intensity with increasing total intensity of the complex sound.

The data show that the greater the combined intensities of the components of the complex sound, the more the intensity of either component must be raised in order to obtain a given increment of loudness. This may be seen graphically in Figs. 4, 5, 6, 7. The abscissa shows the intensity, in decibels above threshold, for one frequency

component of the complex sound. The frequency of the other component is indicated below each graph and its different intensities are represented by the different kinds of lines. The ordinate indicates the intensity, in decibels above threshold, of the comparison tone which appeared to be as loud as the complex sound. For example, in Fig. 4, point *A* represents a complex sound composed of a 1600 cycle component at 20 decibels above threshold and a 1400 cycle component at 20 decibels above threshold. This complex was judged to be as loud as a 1600 cycle comparison tone 26 decibels above threshold.

It is evident that the greater the value of the intensity selected for the 1400 cycle component, the less is the slope of the curves obtained, *i.e.* the less the increase in loudness for a given decibel increase in intensity. This graphic representation of the data for the complex sounds shown in Fig. 4 bears out the generalization previously made that the greater the intensities of the components of the complex sound, the greater must an increase in the intensity of either component be, for an increase in the loudness of the complex sound.

The sets of curves shown in Figs. 5, 6, 7 are sufficiently like those in Fig. 4 to support this generalization. The slope of the curves in Fig. 7 at the higher intensity values of the 4000 cycle component would appear more gradual if these intensities were expressed as loudness level values. The equal loudness contours, previously discussed (*cf.* Fig. 1), show that for a 1600 cycle tone and a 4000 cycle tone to seem equally loud, the 1600 cycle tone need not be as many decibels above threshold as the 4000 cycle tone. The difference in intensity between the two tones when they are equated in loudness becomes greater the higher the intensity level. From these facts one can explain why the values of the ordinate of the first part of the upper two curves in Fig. 7 are less than in the corresponding curves of the other figures. The 4000 cycle component of the complex predominates, yet for a given number of decibels above threshold its loudness is less than a 1600 cycle tone an equal number of decibels above threshold.

If the family of curves in Fig. 7 were replotted using loudness level values instead of decibels above threshold to show the intensity values, presumably the effect of frequency on loudness would be taken into account. Consequently one would expect them to agree quite well with the curves in Fig. 5 since the same complex sounds were used in obtaining these two groups of data. The only difference in the procedure used to obtain the data in Figs. 5 and 7 was the substitution of a 4000 cycle comparison tone for the 1600 cycle comparison tone. Plotting in terms of loudness level values takes the effect of this difference into account. Figure 8 shows the data from Fig. 5 and Fig. 7 replotted in this way.

The abscissa of Fig. 8 represents the intensity, in decibels above threshold, of the 1600 cycle component of the complex sound. The legend below shows the lines representing the different intensity values of the 4000 cycle component of the complex sound. The ordinate represents the loudness level of the comparison tone which appears as loud as any particular complex sound defined on the graph. The Δ's show the values obtained when 4000 cycles was employed as a comparison tone, the *O*'s represent the data when 1600 cycles was the comparison tone.

A study of this graph indicates that when the 4000 cycle component of the complex sound is louder than the 1600 cycle component, the comparison tone has a greater loudness level value if it is 4000 cycles than if it is 1600 cycles. That is to say, the comparison tone, if it is 4000 cycles, must be a greater number of decibels above the 1000 cycle threshold to sound as loud as the complex, than if the comparison tone is 1600 cycles. Note that every pair of curves crosses where the two components are not very different in loudness, and that the curves diverge at the ends. As an example of the phenomenon, consider the two curves represented by the dotted lines. The first point on the upper curve, marked by Δ, shows that a 4000 cycle comparison tone with a loudness level of 28 decibels is judged to be as loud as a complex sound composed of 4000 cycles at 30 decibels and 1600 cycles at 10 decibels. When the comparison tone is 1600 cycles for this

same complex the loudness level is only 22.5 decibels, as shown by the lower of the two curves (marked with *O*). A differ-

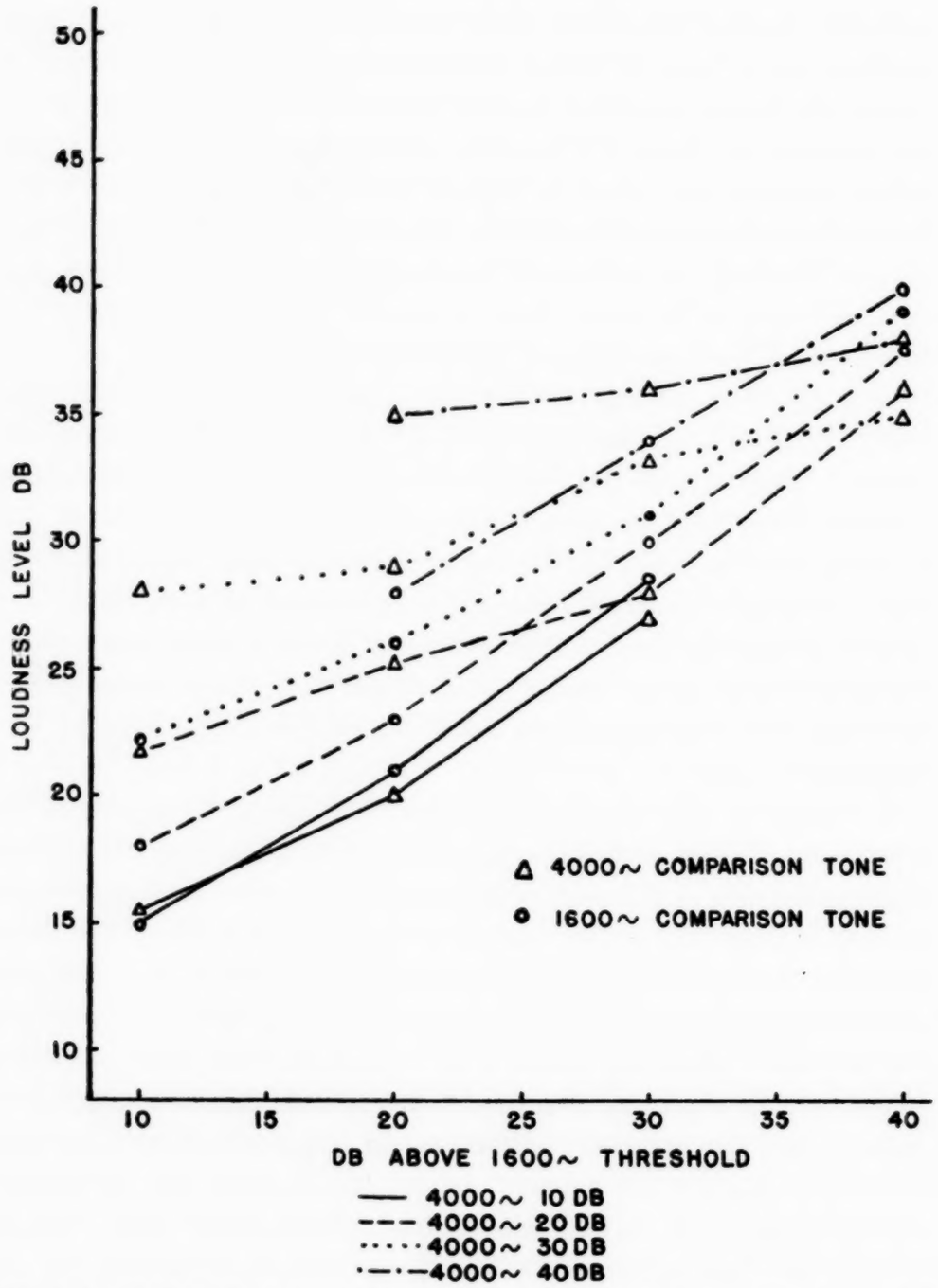


FIG. 8. Effect of pitch of comparison tone on judgment of the total loudness of the complex sound.

ence, similar in direction, but diminishing in amount, is seen as one moves to the right along the two curves until they cross

at the 33 decibel value of the 1600 cycle component of the complex sound. At the next experimentally determined point, where the 1600 cycle component of the complex is louder, the situation is reversed though the absolute amount of the difference is less. That is, the 1600 cycle comparison tone now has the greater loudness level.

While the differences shown by the four pairs of curves are not great, their direction and consistency seem to indicate that the effect may be other than a chance occurrence. When this effect first was noted it seemed possible that it arose from the use of the equal loudness contours to transpose the intensities of the 1600 cycle and 4000 cycle tones into loudness level. That is to say, it was thought that equal-loudness contours, if experimentally determined for the subject, might be different from those found by Fletcher and Munson. In order to test this assumption the subject made judgments directly on equivalent loudness for 1600 cycles and 4000 cycles. The results of these measurements, however, were found to agree fairly well with the values taken from the equal loudness contours of Fletcher and Munson. Apparently, then, there is a tendency for the judgment of the loudness of a complex sound, when defined in terms of a simple comparison tone, to be influenced by the pitch of that comparison tone in some way not previously taken into account.

Comparison of Results with Similar Data from Fletcher and Munson

As part of the basic data from which the loudness function was determined, Fletcher and Munson used results showing the combined loudness level of two tones when heard together. The two frequencies were supposed to be far enough apart so that the stimulated portions of the basilar membrane did not overlap, *i.e.* so that interference between the tones would not be present. In that case, and since each component was of the same loudness level, the combined tones should be twice as loud as either alone. Such data are similar to those on the relative loudness of a given intensity when heard by one and by two ears respectively. These two types of data were

fundamental in calculating the function relating loudness numbers to loudness level. In the present experiment, when the loudness level of the two components of the complex sounds was the same, data comparable to the two-tone data of Fletcher and Munson were obtained. Figure 9 shows the extent to which the two sets of data correspond. The different lines in the legend represent the different frequency combinations. The abscissa shows the loudness level of each of the two components of the combination, while the ordinate shows the loudness level of the two components combined.

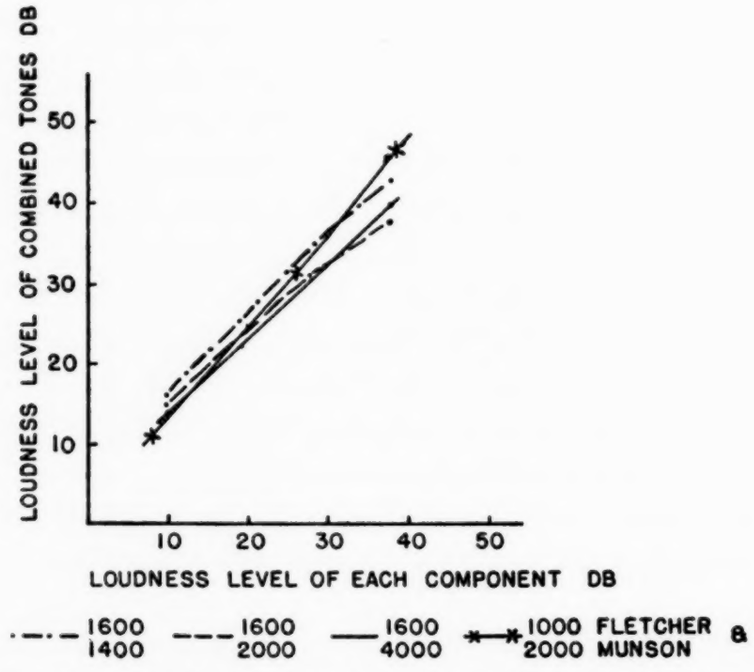


FIG. 9. Comparison of loudness level values for complex sounds of two components with similar data from Fletcher and Munson.

The agreement of the two sets of data is fairly good at the intensities shown, though the curves representing the present experimental data have a slope somewhat less steep than that of Fletcher and Munson's curve. This is particularly noticeable at the higher intensity levels. The fact that the curves for the 1600–2000 and 1600–1400 combinations bend markedly away from the Fletcher and Munson curve at the higher intensities may merely indicate that at these intensities the frequencies are close enough together to interfere with one another. That is, masking occurs for these combinations,

though it does not occur for the 1600–4000 cycle combinations, which continue to follow a straight-line curve.

Masking

Earlier in this paper the masking effects of different frequencies in a complex sound were mentioned. The limited range of frequencies and intensities employed in this experiment make extensive generalizations unwise, but it is true that in a great many of the judgments where one component of the complex sound was 20 decibels greater than the other, the less intense component appeared to make no contribution to the total loudness of the complex sound.

Comparison of Observed Data with Calculated Values

The formula developed by Fletcher and Munson enables one to calculate the loudness level of complex sounds when heard by both ears, if their frequency composition and intensities are known. As has been explained previously, this loudness level value is the intensity of a reference tone which is judged to be as loud as the complex sound when both the complex and the reference sounds are presented to both ears of the subject.

The calculated loudness level value is obtained from the 'loudness number' of the complex sound with the aid of a table showing the relation between loudness numbers and loudness level. If a complex sound is presented to only one ear of a subject, the formula requires that a factor ½ be placed before the loudness number according to the assumption that a sound heard by one ear is only half as loud as when heard by two. Thus the loudness of a complex sound as heard by one ear is obtained in terms of a comparison tone heard by two ears.

In the present experiment the complex sound was presented to the right ear. If the formula for hearing a complex sound with one ear is used, the calculated value is the intensity of the reference tone heard by two ears, which seems as loud as the complex heard by one. The value experimentally

determined in the present experiment is the intensity of a comparison tone heard by one ear. The data on the loudness of sounds when heard by one and two ears respectively will show the increase in the intensity of the calculated value necessary for it to be as loud as the observed value. When this increment is added the calculated value obtained is the same (within the experimental error), as if the formula for two ear hearing had been used for calculation throughout. This indicates that the formula which applies to hearing with two ears applies equally well to a complex sound heard by one ear and equated in loudness to a tone heard with one ear. (*Cf.* **8**, p. 945.) It should also apply where an additional ear is involved to an equal degree in hearing both the complex and comparison sounds. Where parts of the complex sound affect this second ear and others do not, allowance must be made, if it is desired to compare calculated values with those observed empirically.

In the experiment here reported, the ear phone was about seven inches from the right ear. From measurements with a condenser microphone, calibrated to read absolute sound intensities, it was found that the 1400, 1600 and 2000 cycle tones were attenuated approximately 20 decibels by the head, while the 4000 cycle tone suffered about a 28 decibel decrease. Consequently, there are three possible conditions with respect to sound intensity at the left ear in the course of the present experiment.

In condition A, the intensity of the comparison sound and that of each of the components of the complex are so small that no effective sound stimulus reaches the opposite ear. In this situation the observed values should agree well with the calculated values. In condition B, the effective intensity of the comparison tone which reaches the opposite ear is approximately the same as the intensity of the component of the complex sound which reaches this ear. In other words both comparison tone and complex sound are effective equally and so their relation remains the same. Hence here also, there should be fair agreement between calculated and observed results. In condition C, the intensity of the comparison tone

which reaches the opposite ear is larger than that from the complex sound so that it tends to be more effective psychologically, *i.e.* tends to increase the loudness in this ear more than does the intensity of the components of the complex sound. Consequently the total intensity necessary for the comparison tone to appear as loud as the complex need not be so great as if this proportionately greater leakage of sound around the head did not occur.[2]

The loudness levels of the complex sound were calculated according to the formula of Fletcher and Munson as if the left ear were not involved in the loudness balances. Conditions A, B and C were then considered in relation to these calculated values. In the cases where condition A obtained, no further calculation was required. Where conditions B or C held, the data on the attenuation of the different frequencies by the head made it possible to calculate what were the intensities of the various frequencies at the left ear. It was then a simple matter to correct the earlier calculations. The calculated values shown in Tables 5, 6 and 7 were obtained in this fashion.

The first column shows the frequencies and the second column the intensities of the complex sounds. The third column shows the loudness level value of the complex sound as calculated by the formula. The other columns, marked S1, S2 and S3, show the loudness level value of the comparison tone judged to be equal in loudness to the complex. Table 5 shows that the calculated values for the 1400–1600 cycle complexes follow the observed values quite closely. In the 1600–2000 cycle combination the agreement is fairly good except for complexes 7, 11, 12 and 16. In every one of these cases the 2000 cycle component of the complex was the louder. This suggests that when the predominant pitch of the complex is other than 1600 cycles, its influence on the judgment of loudness is not adequately taken into account by the formula. The 1600–4000 cycle combinations lend support to this idea. The calculated results do not correspond

[2] Condition C may also serve to explain the depression of the first part of the upper two curves in Figs. 6 and 7.

TABLE 5

COMPARISON OF OBSERVED RESULTS WITH THOSE CALCULATED ACCORDING TO THE FORMULA OF FLETCHER AND MUNSON

	Freq.	Complex Sound	Calculated Comparison	Observed Comparison 1600		
		Loudness Level Db	Loudness Level Db	Loudness Level S1	Loudness Level S2	Loudness Level S3
1	1600 1400	10 10	13	16	16	17
2	1600 1400	10 20	20.5	22	22.5	24
3	1600 1400	10 29	29	28	29	30
4	1600 1400	19 10	20	22	23	22
5	1600 1400	19 20	24	25	25.6	26
6	1600 1400	19 29	30	30	32	31
7	1600 1400	19 38	39	37	39	38
8	1600 1400	28 10	28	30	31.5	30
9	1600 1400	28 20	30.5	32	34	31
10	1600 1400	28 29	33	34	36	35
11	1600 1400	28 38	40	39	41	40
12	1600 1400	28 48	50	45	48.5	46
13	1600 1400	38 20	40	40	42	40
14	1600 1400	38 29	43	41	43	41
15	1600 1400	38 38	43.5	43	45	44
16	1600 1400	38 48	51	49	52	

TABLE 6

COMPARISON OF OBSERVED RESULTS WITH THOSE CALCULATED ACCORDING TO THE FORMULA OF FLETCHER AND MUNSON

	Freq.	Complex Sound	Comparison Calculated	Comparison Observed 1600		
		Loudness Level Db	Loudness Level Db	Loudness Level S_1	Loudness Level S_2	Loudness Level S_3
1	1600 2000	10 10	13	12	16	15
2	1600 2000	10 19	20	21	20	19
3	1600 2000	10 28	28	27	25	21
4	1600 2000	19 10	20	20	20	22
5	1600 2000	19 19	24	24	24	23
6	1600 2000	19 28	30	28	28	25
7	1600 2000	19 37	38.5	34	32	28
8	1600 2000	28 10	28	28	26	28
9	1600 2000	28 19	29	28	28	28
10	1600 2000	28 28	33.5	30	31	30
11	1600 2000	28 37	42	36	36	32
12	1600 2000	28 46	49	43		37
13	1600 2000	38 19	39	36	38	39
14	1600 2000	38 28	40	36	37	37
15	1600 2000	38 37	44	37	40	38
16	1600 2000	38 46	53			43

TABLE 7

COMPARISON OF OBSERVED RESULTS WITH THOSE CALCULATED ACCORDING TO THE FORMULA OF FLETCHER AND MUNSON

	Freq.	Complex Sound	Comparison Calculated	Comparison Observed 1600			Comparison Observed 4000
		Loudness Level Db	Loudness Level Db	Loudness Level S1	Loudness Level S2	Loudness Level S3	Loudness Level S2
1	1600 4000	10 10	13	14	13	17	15.5
2	1600 4000	10 19	20	16	18	20	22
3	1600 4000	10 27	27	20	21	24	27
4	1600 4000	19 10	20	21	19	22	20
5	1600 4000	19 19	24	23	22	25	25
6	1600 4000	19 27	29	24	26	26	29
7	1600 4000	19 36	37	27	29	29	35
8	1600 4000	28 10	28	29	28	29	27
9	1600 4000	28 19	30	30	29	30	27
10	1600 4000	28 27	34	31	30	32	33
11	1600 4000	28 36	40	32	34	34	36
12	1600 4000	28 45	47.5	34.5	39	37	
13	1600 4000	38 19	40	40	39	40	36
14	1600 4000	38 27	42	41	39	41	36
15	1600 4000	38 36	45.5	41	41	42	38
16	1600 4000	38 45	52	43	43	43	

so closely to the observed values where the 4000 cycle component of the complex is predominant and 1600 cycles is the comparison tone. They do agree, however, where 4000 cycles is the comparison tone (complexes 3, 7, 11). At the higher intensity levels where the 1600 cycle component of the complex predominates, the observed values, when 4000 cycles is the comparison tone, agree less well with the calculated results than if 1600 cycles is the comparison tone. These results are further evidence of the effect of pitch on the judgment of the loudness of a complex tone in terms of a comparison frequency.

Summary

1. The greater the combined intensities of the components of the complex sounds 1600–1400, 1600–2000, 1600–4000 cycles, the more the intensity of either component must be raised in order to obtain a given increment of loudness.

2. The data on the loudness of a complex tone having two components of equal loudness level show fair agreement with similar data secured by Fletcher and Munson.

3. There is a tendency for the judgment of the loudness of a complex sound, when defined in terms of a simple comparison tone, to be influenced by the pitch of that comparison tone in some way not previously taken into account.

4. A comparison of the observed loudness level values of the complex sounds with values calculated according to the formula of Fletcher and Munson shows good agreement. Certain exceptions probably indicate an effect of the pitch of the comparison tone on judgments of loudness.

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THE OVERT BEHAVIOR PATTERN IN STARTLE[1]

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In 1929, Strauss reported the presence of a definite overt behavior pattern in the adult human occurring as a response to a sudden shot stimulus.[2] The response pattern consisted of a forward movement of the head, shutting of the eyes, a characteristic distortion of the features, a raising and drawing forward of the shoulders, an abduction, forward elevation, and inner rotation of the upper arms, bending of the elbows, pronation of the forearms, clasping of the hands, contraction of the abdomen, forward movement of the trunk, bending of the legs at the hips and knees, and accompanying random foot movements.[3] This pattern varied in the degree of its manifestation among subjects and in any one subject from time to time, but its general outline was uniform in all subjects, and was not influenced by the direction from which the

[1] From the Psychological Laboratory of the N. Y. State Psychiatric Institute and Hospital. We wish to express our thanks to Mrs. Edna B. Hunt and Mr. Arthur L. Bernstein for assistance in some parts of the work.

[2] Strauss, H., Das Zusammenschrecken. *J. Psychol. u. Neurol.*, 1929, **39**, 111–231.

[3] *Ibid.*, p. 141. "Bei der aufrecht stehenden Vp. mit dem in Mittelstellung befindlichen Kopf und herabhängenden Armen fanden sich stets, unabhänig von der Richtung, aus welcher der Schall kam, ganz bestimmte Einzelbewegungen als Bausteine des Bewegungsbildes der ersten Phase. Diese sind, wenn wir sie am Körper von oben nach unten gehend aufzählen: Augenschluss, eine ganz charakteristische Verziehung des Gesichtes, Kopfbeugung nach vorne, Hoch- und Vorziehen der Schultern, Abduktion, Vorwärtselevation und Innenrotation des Oberarmes, Beugung im Ellbogen, Pronation des Unterarmes, Handschluss, Kontraktion der Bauchdecken, Rumpfbeugung nach vorn, Beugen der Beine in Hüfte und Knie (natürlich stets zusammen mit der entsprechenden, nie besonders erwähnten Fussbewegung), wozu bei der sitzenden Vp. noch Adduktion und Innenrotation in der Hüfte kommt. Nie treten Einzelbewegungen auf, die einer der hier bezeichneten entgegengesetzt verlaufen, also nie z. B. Streckung von Rumpf oder Kopf."

sound came. The response was very rapid, coming and going in less than half a second. The initial pattern was followed by less rapid secondary responses, viz., orientation to the position of the stimulus, etc., which showed no uniformity among the various subjects. Because of the rapidity of the initial response, it ordinarily escapes the human eye, and must be recorded by a moving picture camera running at ultra rapid speed. Strauss also claimed that this uniform response of the body was symmetrical, that no movements opposed to this general pattern appeared, and that there was no activity of distal parts of the body without activity of the proximal parts. A possible conscious correlate of such a reaction has been observed.[4]

Since so few definite behavior patterns are demonstrable in the field of human emotion, the discovery of a new one is of vital interest. Not only would Strauss' startle response be important as a new behavioral correlate of emotion which might shed much needed light upon the biological and neurological functions involved, but its existence would offer definite hope that the further refinement of our experimental techniques might still reveal material that has eluded previous experimenters. For such reasons, the writers desired to check Strauss' findings.

Procedure

Eleven subjects took part in the experiment. Five of these subjects (three female, two male) were classed as naïve, as they did not know that a shot was to be fired. The remaining six (two female, four male) were not considered as naïve subjects since they either knew of, or suspected the shot stimulus.

The subjects were photographed against a black drop curtain ruled into two inch squares by crossing bands of white. The women wore one piece bathing suits, the men short trunks. Thus there was a minimum of clothing to interfere with bodily movement, and whatever movement occurred was clearly discernible. To guard against eye strain from the strong artificial light used for the photography, each subject wore a black domino mask. Electrodes for electrocardiographic recording were attached to the trunk in such a way as not to interfere with bodily movement and were connected to a string galvanometer in an adjoining room.

The photography was done with a Bell and Howell motion picture camera running at a speed of sixty-four frames per second. Flood lights afforded adequate illumina-

[4] Hunt, W. A., The pressure correlate of emotion. *Amer. J. Psychol.*, 1931, **43**, 600–605.

tion. The stimulus used was the explosion of a blank cartridge in an electrically fired revolver. It was placed above, behind, and slightly to the right of the subject. Firing the revolver activated a pointer whose movement registered in the picture.

The subjects were asked to stand facing the camera with their arms at their sides. The naïve subjects were told that the experiment was concerned with the effect of various postures on the heart rate. The setting rendered this explanation satisfactory. When the subject had been posed, the camera was started and then the revolver was fired. About 2 seconds after the shot, the camera was stopped. Although this procedure was repeated three times with each subject, only the reactions to the first, unexpected shot were used.

In our analysis of the records, the pictures were projected as stills, several frames at a time, on a large screen. Two judges, working independently and at different times, estimated whether or not movement was present and, if present, of what sort and extent it was. Before beginning, the judges were practiced in the task. Each judge assigned 1 point for noticeable movement and 2 points for considerable movement. Thus a typical score might run from 1, or a decision of noticeable movement by one judge, up to 4, or a decision of considerable movement by both judges. In practice, we found very good agreement among the judges. The scoring system was rough, but it was felt that any attempt at finer differentiations would have been unwise in view of the subjective nature of the judgments. In this preliminary experiment the writers were primarily concerned with the demonstration of the existence of the response, and only incidentally with its quantitative characteristics.

Results

Eight of the eleven subjects showed some degree of the typical reaction illustrated in Fig. 1. As Strauss has pointed out, not all the elements are present in each subject, but the general pattern is clearly recognizable and uniform for the group. With the three subjects who did not show the response, there is of course the possibility that some elements of it were present but not sufficiently developed to be noticeable. Table I summarizes these results. The facial pattern was not considered here, as it is intended to make this the subject of a further investigation. Contraction of the abdomen was also omitted as this could not be checked by our method of recording. The low frequency of hip and knee flexing is no doubt due to the difficulty of detecting fine movement in these joints. Since the foot movements were not uniform, no attention has been paid to these. Strauss' description of the head movement as 'Kopfbeugung nach vorne' is a bit ambiguous as the foreward motion of the head may be accompanied by either a raising or lowering of the chin. There would seem to be no difference between the

naïve and the sophisticated subjects. One would expect this agreement in the case of such a rapid, involuntary response. No sex differences were found.

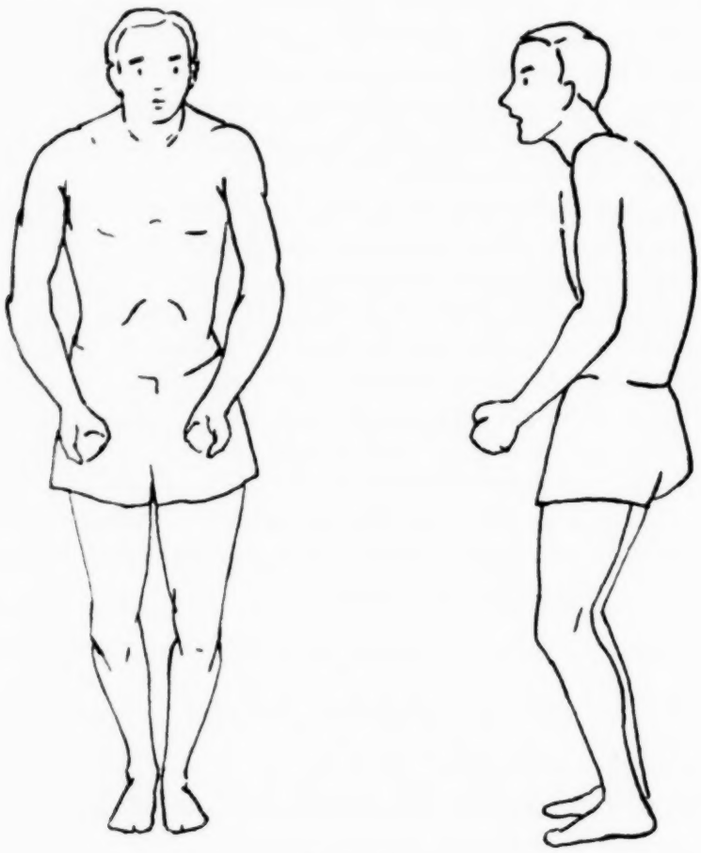

FIG. 1

Examination of Table I does show, despite Strauss' claims to the contrary, that movements opposed to the general pattern do occasionally appear. Such an opposed movement is illustrated by an extension of the fingers which is directly counter to the usual closing of the hand. These opposed movements, however, are relatively infrequent.

A further inspection of Table I shows that there are discrepancies between the response of the right and left halves of the body (R and L in the table). This asymmetry is further shown in Table II, where the letters indicate the various subjects. It is of three kinds: absolute asymmetry, in which one side shows the response while the other does not, relative asymmetry, which is merely a quantitative difference in response, and opposed asymmetry, in which one side shows

TABLE I

		Naïve 5 O's, 3 Showing Reaction				Non-Naïve 6 O's, All Showing Reaction			
		Number Showing Movement	Average Amount of Movement	Number Showing Opposed Movement	Average Amount of Opposed Movement	Number Showing Movement	Average Amount of Movement	Number Showing Opposed Movement	Average Amount of Opposed Movement
Head		3	2.7	—	—	3	2.3	—	—
Shoulders	R	3	2.3	—	—	3	2.0	1	1.0
	L	3	2.3	—	—	2	2.0	1	1.0
Upper arms	R	2	2.0	—	—	1	3.0	3	2.0
	L	2	2.0	—	—	1	3.0	3	2.3
Elbows	R	2	2.0	—	—	2	2.0	—	—
	L	2	1.5	—	—	4	1.5	—	—
Forearms	R	2	2.0	—	—	3	1.7	—	—
	L	1	3.0	—	—	2	1.5	—	—
Hands	R	2	2.0	—	—	1	2.0	2	1.5
	L	2	2.0	—	—	3	1.7	1	1.0
Trunk		2	1.5	—	—	3	3.0	—	—
Hips	R	1	1.0	—	—	2	1.0	—	—
	L	—	—	—	—	2	1.0	—	—
Knees	R	1	1.0	—	—	3	1.7	—	—
	L	1	1.0	—	—	3	1.7	—	—

TABLE II

	Observers Showing Absolute Asymmetry	Observers Showing Relative Asymmetry	Observers Showing Opposed Asymmetry
Shoulders	D		
Upper arms		E	
Elbows	C, E	B	
Forearms	B, C		
Hands	C		F
Hips	B		
Knees			

an opposed movement, such as the closing of the left hand and opening of the right. Our results certainly do not establish the claims of Strauss for the symmetry of the reaction, although the instances of asymmetry are not numerous.

Table III shows that Strauss' further claim that there is no movement of distal parts without movement of proximal parts, *i.e.* closing of the hands without pronation of the forearms, etc., was not substantiated. Apparently this does happen at times.

TABLE III

Movement of Distal Parts without Movement of Proximal Parts

		Naïve	Non-Naïve
Shoulders	R		C, E
	L		C, E
Upper arms	R		G
	L		D, G
Elbows	R	A	
	L	A	C
Forearms	R		C, G
	L		G
Hands	R		F
	L	B	C, F
Hips	R		
	L		
Knees	R		E
	L	B	E

Because of technical difficulties with the electrocardiograph recording system, complete cardiograms were not available for all subjects. Those which are available fail to show any uniform type of heart disturbance in the reaction. The appearance of rapid electrical waves presumably derived

from skeletal action currents [5] was most common. The presence of such waves in one of the subjects who showed no noticeable signs of the reaction, might suggest its appearance at an implicit level.

The total impression that one gets from viewing the pictures of the presence of this uniform, rapid response in most subjects is stronger than the somewhat artificial analysis of the tables indicates. The importance of such a response for the emotional picture shows the desirability of further work. Additional investigation of the response under conditions allowing more careful quantification is indicated. Many other problems are apparent, such as a careful study of the facial components, the investigation of the effects of habituation and voluntary inhibition, possible differences in different age groups, the possible appearance of the response in animals, etc.

Conclusions

In general, Strauss' claim that there is a common uniform behavior pattern appearing in response to a shot stimulus is verified. The pattern consists of shutting of the eyes and distortion of the features, a forward movement of the head, a raising and drawing forward of the shoulders, abduction, forward elevation, and inner rotation of the upper arms, bending of the elbows, pronation of the forearms, clasping of the hands, contraction of the abdomen, forward movement of the trunk, bending of the legs at the hips and knees, and random foot movements. Not all of these elements may be present in any one reaction, and the extent of their appearance is not constant. The response is too rapid for the eye, coming and going in less than half a second. Strauss' further claims of symmetry, the absence of opposed responses, and no movement of distal parts without movement of proximal ones are shown to have exceptions. Further work is indicated.

(Manuscript received May 6, 1935)

[5] Landis, C. and Forbes, T. W., The relation of startle reactions to the cardiac cycle. *Psychiat. Quar.*, 1934, 8, 235–242.

THE EFFECT OF SLEEP UPON RETENTION

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There have been but few experimental studies concerned with the effect of sleep upon retention although a good many years ago Foucault (**1**), in considering the data of Ebbinghaus, suggested that the smaller amount of forgetting in his results for the eight to twenty-four hour period might have been due to the greater predominance of sleep. Radosavljevich (**5**) found similar results and interpreted them as indicating that learning efficiency recovers during sleep. While working with G. E. Müller, Heine (**2**) conducted an experiment to determine the effect of sleep and waking on retention as a function of retroactive inhibition. She compared retention after twenty-four hours when syllables were learned just before going to bed with retention when waking intervals were interpolated between learning and sleep. Her results indicated that when learning occurred shortly before sleep there was an average savings in relearning after twenty-four hours of 47 percent, while there was an average savings of only 36 percent when waking intervals were interpolated between learning and sleep. Jenkins and Dallenbach (**3**) attempted to determine what effect intervals of one, two, four, and eight hours of sleep interpolated immediately upon the learning of one list of ten nonsense syllables had upon reproduction in comparison with waking intervals of the same length. On the average twice as many syllables were reproduced after sleep as after waking. The sleep curve stayed practically on a level after the two hour interval, while the waking curve continued to drop. Van Ormer (**6**) repeated Jenkins and Dallenbach's work employing more precise and exact methods. Two subjects were used, each acting as subject and experimenter for the other. The lists were presented on a memory

drum and three lists of twelve syllables each were learned at a sitting. One correct anticipation was the criterion for learning. His results showed that retention as measured by the percent savings method is definitely better after eight hours of sleep than waking, is probably reliable in favor of sleep at four hours, merely suggestive at two, and not at all different at one hour.

The results of these studies show rather definitely that sleep in comparison with waking does have a beneficial effect upon the retention of nonsense syllables. The longest period studied for the effect of sleep on retention has been twenty-four hours. It seemed desirable, therefore, to determine the differential effect of interpolated periods upon retention when learning immediately precedes a sleeping period and when it precedes a waking period during much longer intervals of time.

The purpose of this experiment was to determine the effect of interpolated periods of various lengths upon the retention of nonsense syllables as measured by the percentage savings method, for twenty-four, forty-eight, seventy-two, and one hundred and forty-four hour intervals (1) when the learning is done just before retiring and (2) when the learning is done in the morning upon awakening.

The author served as subject and experimenter for this experiment. The material consisted of hand printed lists of twelve nonsense syllables each. The syllables were selected on the basis of Krueger's (4) list which gives relative difficulties of nonsense syllables according to their tendency to arouse meaningful associations. My lists were equated as to difficulty in this respect. No syllable was used twice in any list except after a long period of time.

The cards were arranged on a table face up with the first syllable covered by a blank card. They were spread out partially overlapping to facilitate turning. The subject turned one card over at a time while repeating audibly the syllable presented. The subject practiced turning the cards until she had developed a rhythm, each list requiring about twenty seconds. The rhythm was checked frequently with a

stop watch to keep it constant. After completing a list, the cards were again arranged as before and the procedure was repeated. Repeating the entire list twice without looking at the cards constituted the criterion of learning. Two lists were learned at each sitting. An attempt was made to control practice effects by learning preliminary series until there seemed to be no noticeable improvement. However, it was found that improvement continued for a considerable part of the experimental series.

The order of arrangement of the different periods of sleep and waking was a modified form of the ABBA order. The first lists were learned in the morning and recalled the next morning, twenty-four hours later; the second two lists were learned at night and recalled two nights later at the end of forty-eight hours. The complete method of arrangement for all the syllables learned was as follows (W denoting learning in the daytime and S denoting learning at night): W, 24 hrs.; S, 48 hrs.; S, 72 hrs.; W, 96 hrs.; S, 96 hrs.; W, 72 hrs.; W, 48 hrs.; S, 24 hrs.; S, 24 hrs.; W, 48 hrs.; W, 72 hrs.; S, 96 hrs.; W, 96 hrs.; S, 72 hrs.; S, 48 hrs.; W, 24 hrs. Three series were learned in this order but the fourth and fifth series contained, in addition, 144 hour S and W periods. Throughout our discussion sleep period means the interval was begun with seven or eight hours of sleep and waking period means that a full day's activity began the interval.

The results obtained for five complete series show that retention is better when learning takes place immediately before a sleeping period than when learning precedes a waking period from the seventy-second hour on. The walking curve, as shown in Fig. 1, is slightly above the other at twenty-four hours and there is very little difference between the two at forty-eight hours. For 72 hours and longer periods, however, the curves show a considerable separation.

The ratio of the difference to the S.E. of the difference has been computed for each time period in comparing the percentage retention between the learning preceding sleeping and that preceding waking. The ratios follow: for twenty-four hours, the ratio is .96, for forty-eight hours, .19, for seventy-

two hours, 2.20, for ninety-six hours, 2.86, for one hundred and forty-four hours, 2.88. The differences from the seventy-two hour period on, therefore, are of rather high significance for

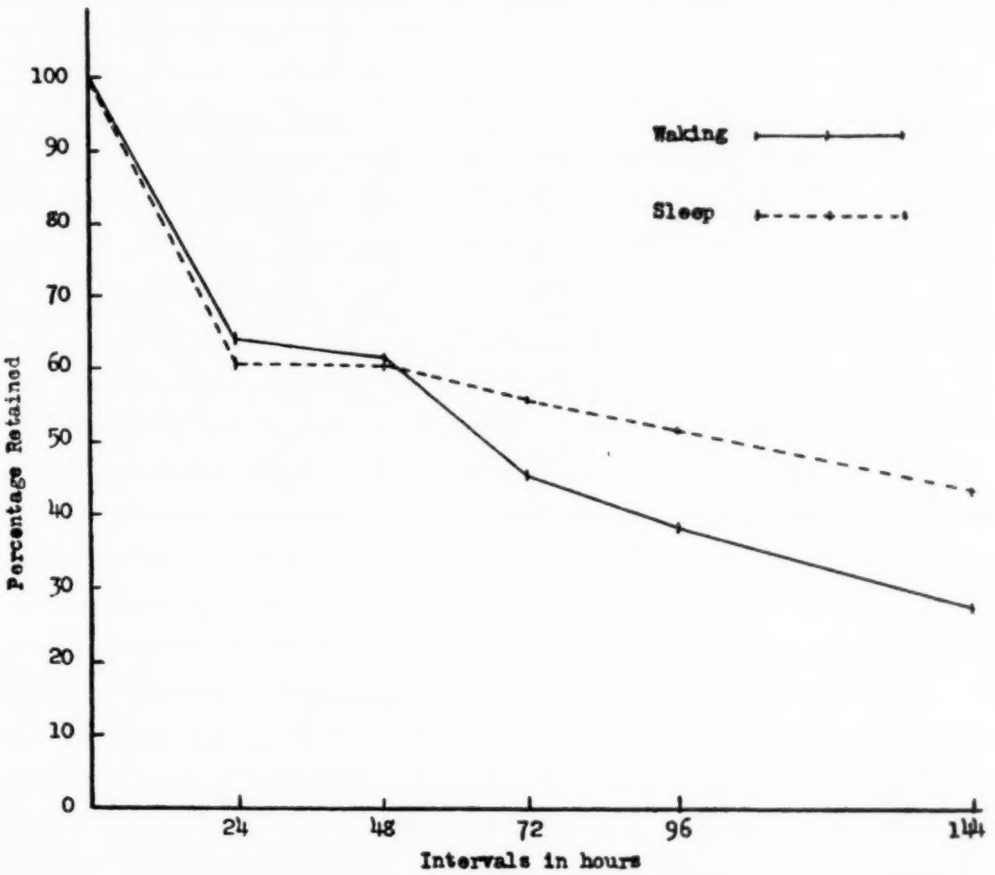

FIG. 1. Percentage of nonsense syllables retained after intervals of sleep and waking

there are approximately 99 chances in 100 that these differences will be in the same direction on future measurements.

Complete data from the experiment is presented in terms of percentage savings in Table I. It will be noted that there is considerable variation in results from one series to another The averages for all series and standard deviations are presented at the bottom of the table. It will be noted that the standard deviations for the waking series are higher than for the sleeping. To determine whether this difference was at all meaningful, the coefficients of variability were calculated. These results are shown graphically in Fig. 2. Although the coefficients of variability increase with the length of the

TABLE I

PERCENTAGE SAVED IN RELEARNING NONSENSE SYLLABLES AFTER PERIODS BEGINNING WITH WAKING AND WITH SLEEP

Series	24 Hrs.		48 Hrs.		72 Hrs.		96 Hrs.		144 Hrs.	
	W	S	W	S	W	S	W	S	W	S
A1	57.5	81.3	66.6	82.4	63.7	60.0	53.0	38.5		
A2	54.6	62.5	44.5	75.0	55.6	50.0	7.2	63.7		
B1	62.5	61.6	82.4	54.6	11.2	66.6	36.4	69.3		
B2	75.0	62.5	66.6	66.6	45.5	57.3	60.0	50.0		
C1	88.9	60.0	62.5	50.0	44.5	50.0	30.0	50.0		
C2	71.5	62.5	55.6	60.0	44.5	66.6	50.0	50.0		
D1	66.6	62.5	69.3	62.5	54.6	66.6	29.9	43.0	16.7	45.5
D2	60.0	44.5	62.5	50.0	40.0	33.3	43.0	37.5	55.6	60.0
E1	55.6	50.0	50.0	50.0	43.2	57.2	30.0	57.2	22.3	43.2
E2	55.6	57.2	57.2	57.2	50.0	50.0	43.2	66.6	16.7	25.0
Aver.	64.8	60.6	61.7	60.8	45.2	55.7	38.2	52.6	27.8	43.4
S.D.	10.4	9.2	10.1	10.5	13.2	9.9	14.3	10.8	16.2	12.4

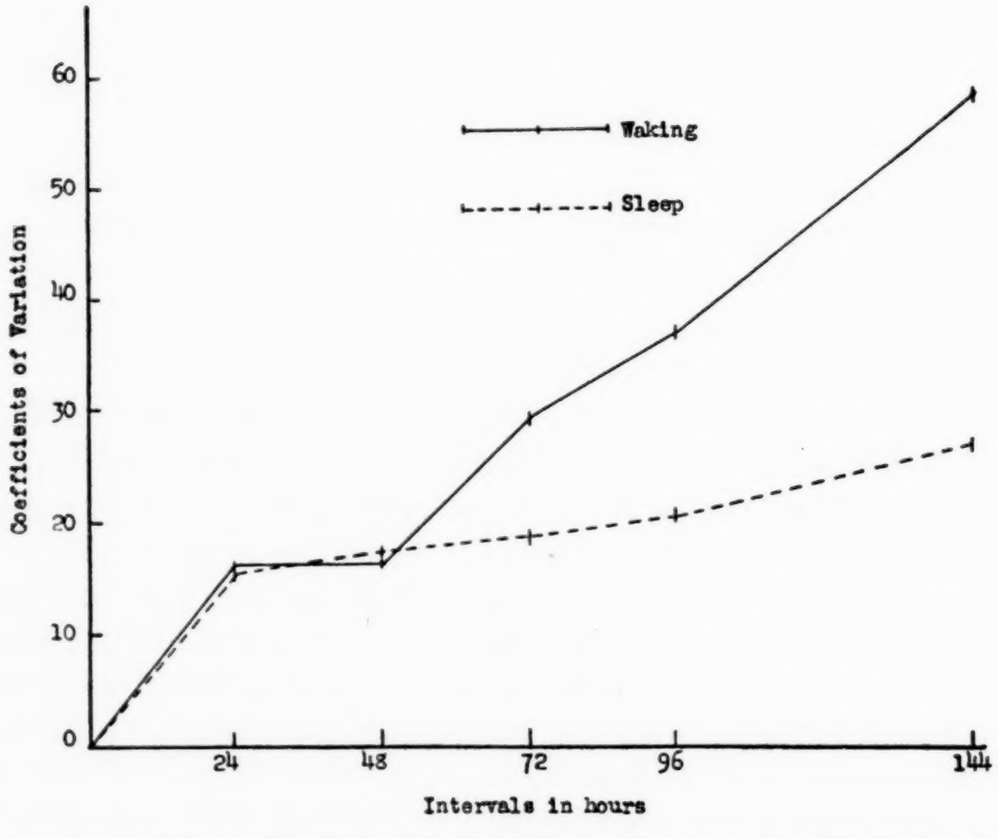

FIG. 2. Coefficients of variation for the percentages of nonsense syllables retained after intervals of sleep and waking.

periods for both sleep and waking, they increase much more for the waking series and this differential increase occurs for the seventy-second hour and longer periods. It should be noted that this difference in variability between sleep and waking periods coincides with the more reliable differences in retention.

The fact that sleep has a beneficial effect upon retention for the longer intervals of time is fairly evident from the data, although the results in this experiment do not agree with those found by Heine for the twenty-four hour intervals. The data also appear to contradict the results found by Jenkins and Dallenbach as well as those found by Van Ormer, but this contradiction may be more apparent than real. It will be recalled that relearning in their experiments occurred immediately after either an entire sleeping or waking period. In this experiment at least twenty-four hours elapsed before the relearning took place, and, of course, only seven to eight of the twenty-four hours were occupied by sleeping. Why there is ever increasing difference in favor of the sleep series beyond seventy-two hours is difficult to understand. If retroactive inhibition causes the difference between the sleep and waking series, the degree of inhibition due to waking activities must affect the retention of material following a waking period to a greater extent than it affects that following a sleep period for the longer time intervals.

Criticism of the experiment can be raised on several grounds. First, only one subject participated in the learning. Second, a controlled method of presenting the lists such as a memory drum was not used. Third, only five series of two lists each were learned altogether. The results might have been different had more subjects participated and more series been learned. On the whole, however, the trends in the data are fairly consistent. Certainly the experiment should be repeated.

In summary we may tentatively say that learning nonsense syllables before sleeping appears to have a beneficial effect upon retention as compared with learning immediately upon awakening for periods of 72, 96, and 144 hours but not

for 24 and 48 hours. When the difference in retention between sleeping and waking periods became reliable (from 72 hours on) the relative variability for the waking period became markedly greater than for the sleeping period.

It would seem that, when both sleep and waking occur before retention is tested, the differential effect from beginning the period by sleep or by waking appears only sometime after 48 hours. The effect is definitely present at the end of the 72 hour and longer periods.

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THE COGNITIVE ASPECTS OF MOTOR PERFORMANCES AND THEIR BEARING ON GENERAL MOTOR ABILITY

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Investigations in the field of motor ability have shown rather consistently that the rank an individual holds among his fellows in one motor performance is to but a slight degree indicative of the rank he may hold in another motor performance. The findings of numerous researchers, reviewed by Seashore,[1] have indicated that motor performances of both laboratory and non-laboratory character are correlated positively but only to a degree represented by the median figure of .25. A concensus of opinion that motor ability is not general but specific is the resultant of such findings.

It has been shown by the writer,[2] however, that this general opinion is apparently dependent on an equally general tendency among researchers to permit the factors which enter into determining the efficiency of the observed performances to operate in a fashion which is largely uncontrolled and unknown. This was demonstrated by the finding of very high intercorrelations between serial motor performances under conditions such that only the muscle group employed varied from task to task, other possible differential factors being experimentally equated in the several tasks. Correlations between four such performances ranged from .84 to .96. These results were interpreted as indicating the presence of general motor ability across the four performances tested.

Although a general ability has therefore been demonstrated

[1] Seashore, R. H., Individual differences in motor skills, *J. Gen. Psychol.*, 1930, **3**, 38–65.

[2] Campbell, M., The 'personal equation' in serial pursuit performances, *J. App. Psychol.*, 1934, **18**, 785–792.

for four motor performances, no evidence was secured with regard to what variable factor was responsible for the consistently low correlations between motor tasks which have led other investigators to deny the existence of general motor ability. However, a means whereby such information could be obtained was established. Using the four performances previously investigated one may permit any factor which had formerly been equated in each task to vary from one task to the next. An observation of the reduction in the size of correlations thus brought about will give an indication of the importance of variation in this factor across performances for the finding of general or specific motor ability. The beginning of such an experimental analysis is presented in the present research.

The data from the original experiment gave evidence that the element of 'muscle group employed' when allowed to vary across performances, does not operate to cause large reductions in intercorrelations between those performances. Four muscle groups, represented by the two arms and the two legs, were used in this experiment. In each of the four tasks a different one of these limbs was employed in operating an apparatus which called for serial pursuit behavior.[3] Four light bulbs were mounted in a diamond shaped arrangement on a board directly in front of the subject. The distance from one bulb to another at the opposite corner of the diamond was approximately 4 inches. That part of the board enclosed by the bulbs was removed leaving a diamond shaped aperture 3 inches in width. An electric key was mounted in each corner of this aperture so that for each light bulb there was a corresponding key immediately adjacent to it. The observer thrust a baton through the aperture and reacted to an illuminated bulb by striking that key adjacent to it. No more than one bulb was illuminated at a time. The action of the subject in striking the key served to activate the instruments so that a change in the illuminated bulb was made automatically and immediately thus presenting the

[3] Campbell, M., Instrument for measuring the serial reaction-times of large muscle groups, *Am. J. Psychol.*, 1934, **46**, 481–483.

subject with a new stimulus and a new key to be struck. The succession of stimuli was made a random one so as to preclude the possibility of learning which might occur on repeated trials. Time limited trials were taken in this fashion, the subject's score being recorded automatically as the number of correct strokes made during the period of the trial. This produced a measure principally of speed of serial pursuit behavior there being but very small need for accuracy of movement due to the instrument's construction.

In these performances it is apparent that the eye is used throughout as the organ through which the subject becomes aware of changes in the environmental situation which call for corresponding responses on his part. One may wish to know, therefore, the extent to which the general ability found is due to the use of this particular sense organ. Would similar results have been found had auditory instead of visual stimuli been used? Furthermore, to what extent is this found general ability a function of the equation of sense organ from task to task? Would the same high correlations have been found had the eye been used in one performance and the ear in another? The present experiment limits itself to the attempt to answer these two questions.

The Experiment

For the purposes outlined above 4 electric buzzers were added to the apparatus. These were so connected into the electrical circuit that by throwing a switch they could be inserted functionally into the places of the corresponding lights. Each buzzer was affixed to the wall about 4 feet out from its corresponding light bulb and on a line from the center of the aperture through the bulb. When these buzzers were used as stimuli the overt movements required of the subject were identical with those required when the lights were stimuli. The response to a light or a sound from the left was a blow with the baton on the left key, a sound or light from below was responded to by striking the lower key, etc. Preliminary work with the buzzers made it apparent that the upper and lower sounds could not be distinguished for direction. As a consequence these two were made to vary somewhat in pitch and loudness from each other and the other two in order to facilitate identification.

Eighty male sophomores taken from psychology classes at Stanford University acted as reagents in this experiment. This group performed two tasks which will be referred to hereafter as Experiment 1 and Experiment 2. Experiment 1 consisted, in effect, in a repetition of the original experiment. The only change introduced was a reduction in working time from 15 trials of 20 seconds each to 10 trials of 15 seconds each, a 50 percent decrease in total working time. Experiment 2 was a repetition of

Experiment 1 with the use of auditory stimuli in place of the visual. In both tasks the four performances were varied in the following manner. One-fourth of the subjects took the first trial with the right arm, the second with the left leg, the third with the left arm and the fourth with the right leg, repeating this series until 10 trials were completed for each limb. One-fourth of the subjects began the series with the left leg, one-fourth with the left arm and one-fourth with the right leg. In this manner any advantage or disadvantage accruing to performances due to their firstness, secondness, thirdness, or lastness was evenly distributed over the four performances.

In Table I may be seen the data obtained from Experiment 1 in which the stimuli were visual in nature. The score used for any particular performance is the average of all 10 trials. The reliability coefficients were obtained by correlating the average of the odd numbered trials with the average of the even numbered trials by each respective limb, the resultant coefficients being stepped up by the Spearman-Brown formula to give the reliabilities of the total performances. The correlation coefficients uncorrected are entered in the lower part of the table of intercorrelations and, corrected for attenuation, they appear again in the upper part of the table.

TABLE I

EXPERIMENT 1

Serial Motor Performances Using Visual Stimuli

	Right Arm	Left Arm	Right Leg	Left Leg
Mean (10 trials)	33.3	33.1	29.7	29.4
S.D.	2.4	2.4	2.1	2.0
Reliability	.96±.01	.97±.01	.97±.01	.97±.01
Intercorrelations				
Right arm	.....	.97±.01	.84±.02	.90±.01
Left arm	.94±.01	.....	.80±.02	.80±.02
Right leg	.81±.03	.77±.03	.....	.98±.01
Left leg	.87±.02	.77±.03	.95±.01	.....

Considering Experiment 1 as a check on the original experiment it may be seen that the 50 percent decrease in total working time did not serve to decrease materially the reliability of the measures which remains at about .97. While the absolute differences in the mean scores of the 4 performances in Experiment 1 are small, they are statistically significant ($D/\sigma D$ = 3 or greater). The critical ratio between

the right arm performance and the left leg performance is 32.5. The size of the intercorrelations to be seen in Table I corroborates the evidence from the original experiment that a general ability has been tapped in these performances.

It may also be noticed that, as in the original experiment, one may divide the correlations into two groups on the basis of size. These groups correspond to groupings among limbs made on a functional or an anatomical basis. That group containing the larger correlations has two members, right arm vs. left arm and right leg vs. left leg. The second group, containing the somewhat smaller figures, has all the correlations representing an arm performance versus a leg performance. In addition there exists a difference in size of correlations within the second group not present in the original experiment. Leg correlations with the right arm are slightly larger than with the left arm.

In Table II may be seen the data secured from Experiment 2, the second task performed by the group of eighty sophomores. Experiment 2 was apparently a repetition of Experiment 1 in all respects but one, auditory stimuli being employed

TABLE II

EXPERIMENT 2

Serial Motor Performances Using Auditory Stimuli

	Right Arm	Left Arm	Right Leg	Left Leg
Mean (10 trials)	18.9	18.8	18.1	17.8
S.D.	2.2	2.2	2.2	2.2
Reliability	.96±.01	.96±.01	.96±.01	.96±.01
Intercorrelations				
Right arm	.....	.99±.01	.99±.01	.96±.01
Left arm	.95±.01	.....	.97±.01	.96±.01
Right leg	.95±.01	.93±.01	.....	.99±.01
Left leg	.92±.01	.92±.01	.95±.01	.....

instead of visual. Actually other differences were present in Experiment 2 as will be shown later. The figures in Table II were prepared and are to be read in the same manner as were those of Table I. Uncorrected correlation coefficients are placed in the lower half of the table of intercorrelations

and the same coefficients, corrected for attentuation, appear again in the upper part of the table.

It may be noted from Table II that the reliability of these auditory performances is quite high being at .96. Intercorrelations between the 4 performances approach closely the limit set by the reliability coefficients. In addition one may note that differences in size between groups of correlations, noted in Experiment 1 and to a lesser degree in the original experiment, have practically disappeared in Experiment 2. The introduction of auditory stimuli in place of visual also served to reduce the mean scores by approximately one-half or, in other words, made the performances about twice as difficult. It is apparent, however, that the use of visual stimuli in the original experiment was not essential to the finding of high correlations between serial motor performances. Even higher correlations were obtained from the performances requiring the use of auditory stimuli in Experiment 2.

In Table III are shown the cross correlations between performances in Experiment 1 with visual stimuli and performances in Experiment 2 with auditory stimuli. These coefficients are corrected for attenuation.

TABLE III

EXPERIMENT 1 VS. EXPERIMENT 2

Motor Performances Differing in Sense Organ Employed

	Right Arm	Left Arm	Right Leg	Left Leg
Right arm	.45±.06	.44±.06	.40±.07	.40±.07
Left arm	.45±.06	.42±.06	.37±.07	.37±.07
Right leg	.51±.06	.49±.06	.41±.06	.45±.06
Left leg	.46±.06	.40±.07	.37±.07	.41±.06

The median coefficient in Table III is .42. This is a considerable reduction from the correlations to be seen in Tables I and II, whose medians are in the neighborhood of .96. Furthermore, groupings of correlations such as were noticed in Tables I and II and in the original experiment are not found in Table III. Apparently, the factor of difference in sense organs employed is of considerable importance in determining the size of correlations between serial motor

performances as well as the differences in size between particular correlations or groups of correlations.

Discussion

It seems from the data so far presented that the existence of a difference between two motor performances in the sense organ employed will result in a comparatively low correlation being obtained between those two performances. Also the effect is so great as to entirely conceal small differences in size of correlations associated with differences in the muscle groups employed. That these effects are attributable to this factor becomes very dubious, however, upon further analysis of the experiment. It was assumed that the only difference between the performances of Experiment 1 and those of experiment 2 was the bare element of sense organ employed. That Experiment 2 failed to limit itself to this single variation is apparent if some other aspects of the two types of performance are taken into account.

It has already been noted that auditory performances, were approximately twice as difficult as visual performances, as indicated by mean scores. This difference is accountable to several factors which may be distinguished in the test situations. First, in Experiment 2, while the sounds were loud enough to be heard easily, it was difficult to distinguish them for direction which was the determining cue for reaction. This factor was minimized to some extent by making the sounds vary somewhat in pitch and loudness. While this procedure results in speeding up the performance it is at once apparent that the task has been changed thereby, from Experiment 1 to Experiment 2, over and above the change in sense organ employed. In Experiment 2 the stimuli were identified to an unknown, but probably large, degree on the basis of their qualitative differences. In Experiment 1, on the contrary, the stimuli had to be identified entirely on the basis of position since they were qualitatively the same. The task of identifying a sound on the basis of quality and translating them into positional character probably accounts for a considerable part of the reduction in mean score found in

Experiment 2. The difficulty of identifying the auditory stimuli may also have been increased by the noise of the instrument in operation for which there was no comparable effect for the visual stimuli.

Another factor involved was the difference in the structure and function of the two sense organs. The observer was able to see at all times all the positions taken by the light due to their proximity and the two dimensional character of the retina. Subjective reports from the observers indicated that successive lights seemed to move from one position to the next within the field of vision. The task, according to the observers, took on the character of 'following' the light through the up, down, right, left 'pattern.' Each stimulus was identified in turn in terms of its 'up-ness,' 'down-ness,' 'right-ness,' or 'left-ness' from the previous position. For the sound stimuli of Experiment 2 a totally different situation existed. The auditory sense organ is not spread out in two dimensions as is the retina and air waves may travel around corners which light ordinarily does not do. Consequently the sound stimuli could not be identified immediately in terms of their directional relationship to their predecessors and there were no 'following' reports from the observers. Each stimulus appeared first as a sharp break from the preceding stimulus, a different sound, the process having no character of 'progression from —— to ——.' This new sound was identified in terms of the learned position associated with a sound of that quality. The performance with auditory stimuli was, therefore, a succession of relatively discrete reactions to unrelated, non-'patterned' stimuli while the performance with visual stimuli was a continuous progression 'from —— to ——' between inter-related, 'patterned' stimuli.

It would seem that the effect observed in Table III, in which cross correlations between visual and auditory performances are tabulated may therefore be ascribed not so much to the simple difference between performances in sense organ used but to the large difference apparent in the cognitive processes of discrimination necessary for the performances.

The reduction of the median correlation coefficient of .96 observed in Tables I and II to the median figure of .42 in Table III indicates that this difference in cognitive processes between visual and auditory performances varied considerably from individual to individual.

A further indication of the relative importance of the cognitive aspects of these performances was obtained by requiring the motor performance without the necessity of identifying successive stimuli. When the apparatus was so changed that any key would respond at any time the necessity for discrimination of stimuli was removed. Under such conditions and when going through an approximately normal succession of reactions the subject could make a score about double that of the normal visual performance or triple that of the normal auditory performance. The need for the cognitive activity of discrimination therefore brought about a large decrement in speed of reaction and this cognitive involvement was greater for auditory performances than for visual performances.

Taking Experiment 1 as a standard, Experiment 2 and the original experiment may be looked upon as variations and their effects compared. The original experiment varied in that the total working time was 100 percent greater. Differences in results from the standard were therefore the effect of the greater practice in the original experiment. Experiment 2 varied from Experiment 1, the standard, in that auditory stimuli were used in place of visual which resulted in a significant change in the cognitive aspects of the task. It has already been noted that the mean scores obtained in the standard performances were smaller than those obtained in the original experiment, time per trial having been equated. It may be said therefore that greater practice results in higher scores being made. The auditory performances however produced mean scores much lower than those of the standard task. This indicates that greater cognitive involvement results in lower mean scores being made. However, greater practice and greater cognitive involvement, as shown in the respective variations, have similar effects on the size of and

relationships between correlations. Both serve to increase correlations in size. Both bring about a reduction of the differences in size between the groups of correlations, groups which correspond to anatomical or functional groupings of limbs.

Summary and Conclusions

The original experiment which demonstrated the presence of a general ability across four serial motor performances has been repeated and corroborated. Speeded performances which are experimentally equated so that only the element of muscle group employed is permitted to vary produce intercorrelations of approximately + .96.

The size of these correlations between the four motor performances is not dependent upon the fact that the visual sense organ was employed. When auditory stimuli were substituted for the visual no attendant reduction in correlations was observed.

While the introduction of auditory stimuli in place of the visual seemed but a slight change in the task, actually it constituted a very important change. It was found that the cognitive aspects of the task, that is the discrimination and identification of stimuli for direction, the necessary cue for reaction, were apparently quite different although the major limb movements required remained the same. As a result it was found that the correlations between performances differing only in sense organ employed, and therefore in their cognitive requirements, averaged approximately + .42. This is a considerable reduction from the average figure of + .96 for performances between which no such difference in cognitive requirements existed.

The effect of greater practice in a performance is to *increase* mean scores. The effect of greater cognitive involvement in a performance is to *decrease* mean scores. Both greater practice and greater cognitive involvement, when they operate on both of the tasks which are compared, serve to *increase* the size of the correlation and to *decrease* the num-

ber and size of differences between correlations and groups of correlations.

From these findings it may be surmised that evidence for general motor ability would be more favorable if it were obtained from tasks which involved similar cognitive requirements and the data drawn from a point well along on the practice curve. Conversely, it is suggested that small correlations between motor performances are probably a function of the difference in cognitive requirements for the respective tasks as well as the unperfected nature of the performances.

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THE RETENTION OF MENTAL AND FINGER MAZE HABITS

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Introduction.—Is the retention value of skilled or non-verbal habits greater than that of verbal or ideational ones? Strange to say, there have been few attempts to answer this question.[1] In spite of the paucity of evidence based upon direct comparison under controlled conditions, the answer of both psychologist and layman has usually been 'yes' to the above question.

McGeoch and Melton (3) have suggested that the popular generalization *that skilled or non-verbal habits are better retained than are verbal or memorial ones* may be untrue. They offer in support of this view the result of comparisons, under somewhat similar conditions, of retention of three finger maze habits and three nonsense syllable lists after an interval of one week. Later McGeoch (4) compared one of the mazes with one of the nonsense syllables after a period of eight weeks with no intermediate practice or relearning, and also with an interpolated relearning after one week. He also compared retention of a Worden 10-alley stylus maze and a 10-letter Peterson rational learning problem. His results, as he interprets them, all contribute evidence of the superiority, in retention value, of ideational or verbal habits over maze habits. This is true whether retention is measured after either a relatively long (8 weeks) or a short (1 week) period. He cautions that the obtained difference is probably a result of experimental conditions and he infers that if original learning has been truly equivalent, then there is no difference. He does suggest that interpolated practice improves retention of non-verbal habits more than verbal habits.

[1] For a review of the literature dealing with this problem, see McGeoch and Melton (3).

A second experimental venture was that of Freeman and Abernethy (**1** and **2**). In their introduction they refer to the generalization *motor skills or habits are better retained than are so-called ideational or memorial skills.* These experimenters criticize McGeoch's and Melton's procedure because while the two types of learning were directly compared under the same general conditions, yet the two types were not alike as to structure. Their own experimental situation involved two groups of twenty-five subjects in each. One group (unfamiliar with typewriting) learned to type a paragraph using covered keys and a keyboard chart. The second group learned to substitute digits for the letters in the same paragraph. Two correct repetitions was the criterion of learning for both groups. General conditions were approximately the same for both groups. Retention after two weeks was about equal. After ten weeks, retention was greatly in favor of the typewriting group.

In considering the variables to be controlled in order to obtain valid measures of the retention of motor and verbal habits, the experimenter has recognized two types: those of structure inherent in the materials to be learned, and those pertaining to the external conditions under which the learning takes place.

As far as is known, both McGeoch and Melton and Freeman and Abernethy used all standard precautions to control the external conditions. The latter criticize the former because nonsense syllables and finger mazes are not alike as to structure. They believe that their use of typing and substitution of the same paragraph equalizes structural conditions and it presumably follows that their results are more valid than those of McGeoch and Melton.

There are two possible ways of equalizing the structure of the two types of materials: identity is one; equality as to difficulty in the original learning is the other. Now to the extent that there is identity of material the problem ceases to be a problem. Hence we must have materials which are not identical and therefore our only recourse is to equate the two types of material as to original difficulty. To the writer it

seems reasonable to use all possible care in so doing. He believes that, while McGeoch and Melton might have used more care in equalizing the difficulty of their materials, yet they succeeded better than did Freeman and Abernethy. The former compared a relatively simple maze with a short (8 syllables) list of nonsense syllables, while the latter assumed that learning to type a paragraph and also to substitute digits for the letters in the same paragraph equalized the structure of the two tasks. Such an assumption is naive as one realizes when it is considered that introducing a typewriter with its intricate mechanism makes the typing situation very complicated as compared to the substitution.

Perhaps the best single criterion of equality of structure is the amount of difference in original learning scores. If the differences are large, then such factors as distributed practice, inhibitory or facilitating conditioning, fatigue, etc. increase the variability of conditions instead of lessening it and hence make conclusions invalid.

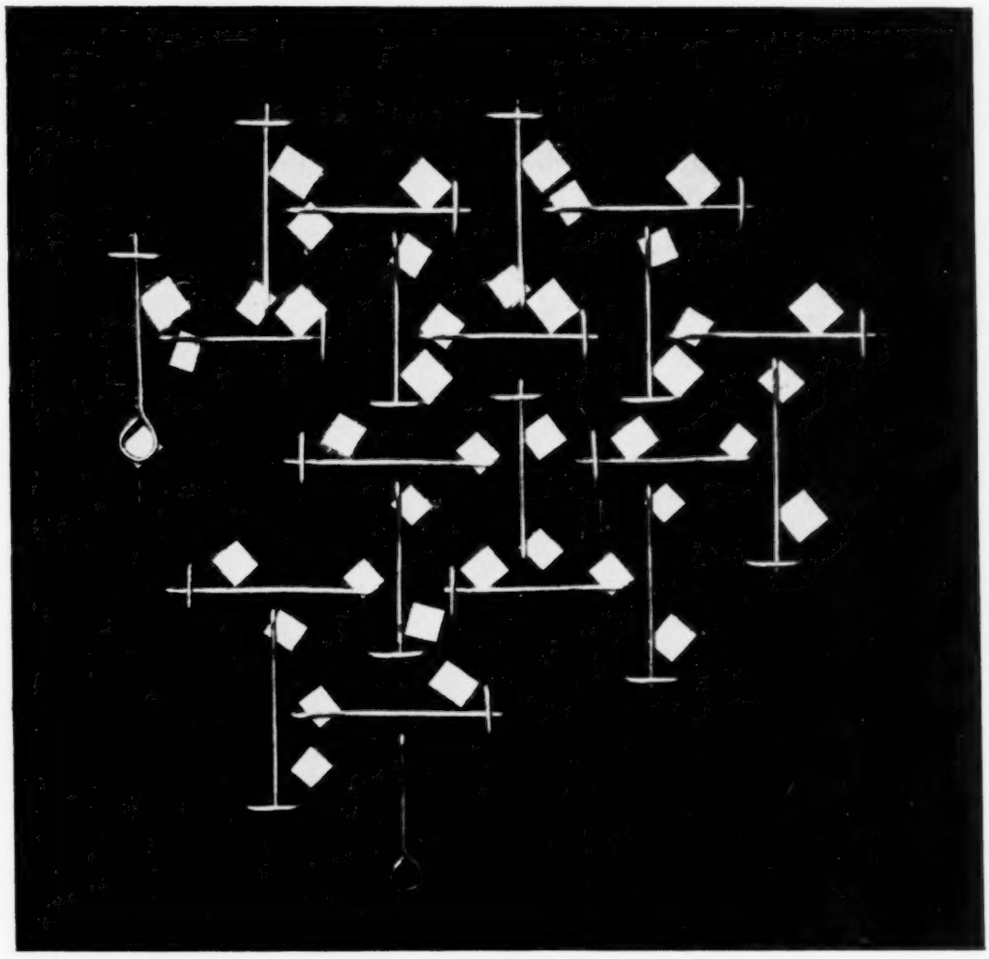
The reason for an earlier statement to the effect that McGeoch and Melton more nearly achieved equality of structure than did Freeman and Apernethy becomes apparent when we find that the former achieved approximate equality between syllable lists and mazes, on the original learning, in both trials and errors while the latter had better than 60 percent more trials in typing than in substitution. McGeoch's and Melton's differences in time were less than half those of Freeman and Abernethy.

Problem.—To compare retention of finger and mental maze habits.

Apparatus.—One wire maze (see illustration). The pattern and style from Miles (5). Wire used is size 16. There are 20 sections to this maze. Each section is approximately 6.8 cm. in length. Each section is divided into 4 parts as follows:

1. Blind alley with wire staple at end	2 cm.
2. Part 1 of true pathway (level)	2.00–2.2 cm.
3. Part 2 of true pathway sloping upward to an abrupt ending	2 cm.
4. Gap between abrupt ending of true path and next section of maze	.5 – .7 cm.
Total	6.5 –6.9 cm.

(This finger maze has a reliability based upon records of 24 subjects of 86. The method used to determine r was as follows. Total number of errors made during the original learning by entering the first 10 blind alleys was compared with those made by entering the last 10. The Rank-Difference method was used and transmuted to r. Then the Brown-Spearman formula was employed to obtain the estimated reliability of the entire maze.)



Entrance to maze.

One mental maze (adapted from Peterson (**6**)) consisting of 20 pairs of nonsense syllables presented verbally as a maze and always in the following order. (Italicized syllables indicate correct choices.)

gik	*yil*	*nal*	wux
pog	mek	*kib*	zut
tev	jom	jat	*tus*
bex	vaf	mez	*vig*
yun	*zug*	*gok*	zad
mip	fuh	niv	*lom*
tuv	*kag*	*kaz*	pid
zoy	*wom*	bep	*paf*
hif	yad	zeg	*nuv*
dag	*zek*	*tife*	gah

(This maze has a reliability of 82. The same method of obtaining r was used as for the finger maze.)

Stop watch.

Sunglasses and cleansing tissue to serve as blindfold. (See Miles (5).)

Subjects.—Approximately 40 subjects were used in equating the two mazes and in standardizing external conditions. The records of none of these appear in the data presented hereafter.

Twenty-four subjects were used. All were college students and with two exceptions they were upper classmen and graduate students. The two exceptions were college freshmen.

All subjects were asked to report a second time but no time was set and they were told that the problem would be a different one. This prevarication was indulged in in order to remove any stimulus for rehearsal.

The subjects relearned at intervals ranging from 44 to 62 days. The median interval was 51 days. The distribution of intervals between the two learning periods was as follows:

TABLE I

TABLE SHOWING NUMBER OF DAYS INTERVENING BETWEEN ORIGINAL LEARNING AND RELEARNING FOR EACH OF 24 SUBJECTS

No. of Days		No. of Days		
62	1	52	2	
61	0	51	4	
60	0	50	2	Mode = 51 days
59	1	49	1	
58	0	48	2	Median = 51 days
57	0	47	2	
56	1	46	1	Average = 50.9 days
55	2	45	1	
54	2	44	2	
53	0	Total	24	

Directions to Subjects

Instruction Preliminary to Both Mazes.—You probably know something concerning the nature of mazes. They consist of numerous connected pathways. Some of these pathways lead to a goal or the end of the maze; others lead nowhere: they are blind alleys. The problem is to begin at the entrance to the maze and proceed to the goal without entering any blind alleys. You may have to try several times. When you succeed twice in succession you will have learned the maze. This is not an intelligence test. There will be no unpleasant stimulation.

Instructions Preliminary to Mental Maze.—This maze is a mental maze. I want to know how quickly you can learn it. Instead of walking through the pathways, as a rat would be required to do, you will have to learn to choose the correct route by means of nonsense syllables or names which are used to designate different parts of the true path as well as the blind alleys.

When I pronounce two names, as for example, *flig-plik*, simply choose one. If your choice is correct I will immediately pronounce two more. If you choose the wrong name, I will repeat the correct one. Repeat this correction after me.

The syllables are always presented in the same order and, as far as possible, in the same tone. However, if there are changes in the tone of the speaker they should have no significance for you.

When you can choose the right names twice, consecutively, without error, you will have learned the maze.

Let us practice on a sample maze until you understand what you are to do.

Instructions Preliminary to Finger Maze.—Here is a wire finger maze. I want to know how quickly you can learn it. As you run your finger along the wire you will, on various occasions, encounter a section which gradually rises for a short distance and then ends abruptly. A very short distance beyond this abrupt ending is another wire which runs at right angles to the former. Follow along this wire until you come to another slope leading to an abrupt ending and another wire pathway a short distance beyond. Never retrace over the short gap and the abrupt ending. If your finger hits one of the staples, you will know that you are in a blind alley and that you must retrace this one section.

Touch only the one finger. Should any other part of the hand touch, an error will be counted. When you can trace the maze twice, consecutively, without entrance to blind alleys, you will have learned the maze.

Here is a sample maze on which we will practice until you understand what you are to do. It is better to proceed at a smooth, even rate; that is, neither fast nor slow.

Method.—'Directions to Subjects' gives much of the method. It is essential to realize that we have attempted to standardize directions, structure of the mazes, etc. so that conditions will approach equality in all respects save that of degree of overtness of the learning activity.

Naturally, equality can only be approached; it can never be completely achieved. Here are some of the ways in which we sought equality of conditions:

1. Each maze is composed of 20 parts. Hence the maximum number of errors possible on both mazes is 20.

2. Both mazes were relatively unfamiliar to all subjects.

3. Both problems were presented as mazes.

4. The same criterion of learning (two consecutive successful trials) was used for both mazes.

5. The same subjects learned each maze under approximately similar conditions and at the same sitting. 13 subjects learned the finger maze first, followed immediately by the mental maze, while 11 reversed the order.

6. Each maze involves a pattern of approximately equal complexity. The order of the nonsense syllables remained as above.

7. The subjects were blindfolded while learning each maze.

8. Errors were immediately announced: in the finger maze learning by contact with a staple across the pathway, in the mental maze by the experimenter's repeating the correct syllable. The structure of the mazes prevented retracing toward the entrance.

RESULTS

TABLE II

AVERAGE SCORES MADE BY 24 SUBJECTS IN FIRST LEARNING OF MENTAL AND FINGER MAZES COMPARED WITH AVERAGE SCORES ON SAME MAZES 51 DAYS LATER

Trials

	Mental Maze			Finger Maze		
		$\sigma_{dis.}$	σ_m		$\sigma_{dis.}$	σ_m
1st learning	17.667	5.404	1.102	20.458	12.076	2.464
2nd learning	10.083	4.903	1.000	12.625	8.112	1.655
Savings	7.584	5.603	1.142	7.833	7.259	1.481
Percent of savings	43.14	....	....	38.29	....	.....

Time (in seconds)

	Mental Maze			Finger Maze		
		$\sigma_{dis.}$	σ_m		$\sigma_{dis.}$	σ_m
1st learning	1306.375	479.269	97.810	921.667	571.865	116.694
2nd learning	649.167	351.292	70.692	432.167	373.673	76.259
Savings	655.208	441.983	90.200	489.500	305.038	62.252
Percent of savings	50.30	.....	.....	53.53	.....	.....

Errors

	Mental Maze			Finger Maze		
		$\sigma_{dis.}$	σ_m		$\sigma_{dis.}$	σ_m
1st learning	69.958	30.253	6.176	65.625	48.017	9.799
2nd learning	23.833	11.013	2.247	26.958	24.948	5.091
Savings	46.125	27.737	5.660	38.667	30.387	6.201
Percent of savings	65.28	.....	.....	58.29	.....	.....

TABLE III

COMPARISON OF MENTAL AND FINGER MAZES IN TERMS OF PERCENTAGE SAVED IN RELEARNING AFTER 51 DAYS

	Trials	Time	Errors
Mental maze	43.14	50.30	65.28
Finger maze	38.29	53.53	58.29
Difference in favor of mental maze	4.85	−3.23	6.99

CONCLUSIONS

These results favor the mental maze in terms of errors and trials; the finger maze in terms of time. Obviously the differences in percent of savings is in no case large enough to be conclusive.

This writer interprets the above data as supporting the contention that verbal habits are retained equally as well as are non-verbal ones, and that there is perhaps no difference in the degree of retention of the two kinds of habits when original learning as judged by common criteria has been equal.

(Manuscript received January 21, 1935)

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THE BASIS OF THE FLICKER IN THE VISUAL FIELD SURROUNDING THE TEST-OBJECT

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The experiments in the present report follow from the common observation that when a bright spot on a dark field is intermittently illuminated (made to flicker) the dark field itself is seen to flicker also. The fact that the latter does flicker, we find to be due to the intermittent illumination of the retina outside of the image by scattered light rather than to some sort of nervous interaction between illuminated and non-illuminated parts of the retina, or to contrast phenomena. Our experiments were therefore made to test the amount or effectiveness of entoptic[1] scatter of light, a factor often merely mentioned but seldom used in accounting for any characteristic of visual phenomena. Its existence and effectiveness in other respects however have been demonstrated in previous experiments (see discussion).

Technique

The present paper gives a comparison of the *critical frequencies* [2] for flicker as seen in the test-object and in the visual field surrounding it. A range in both intensity and area of test-object was used. In part of the cases the test-object was fixated and in the others its image fell on the optic disc or on the opposite side of the retina equally distant from the fovea. Two sets of apparatus for producing intermittent stimulation were used. The one consisted in a homogeneously illuminated field of opal glass 6″ × 6″ upon which circular apertures of various sizes could be placed to serve as the test-object. Behind the screen at a converging point in the optical system, a revolving disc was

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[1] By entoptic scatter is meant the light that falls on the retina outside the image of the light source and arises mainly from the reflection from the image and to some degree from light admitted through the sclera. It is to be distinguished from the dispersion in the media which produces an illumination immediately surrounding the image.

[2] The critical flicker frequency is the minimum rate of intermittent stimulation which will produce a continuous sensation.

located, having equal sized open and solid sectors. One range of intensities had the limits of .075 c/ft.^{2} to 7 c/ft.^{2} controlled by the current in the lamp circuit, while by the removal of the opal glass screen, each setting in the scale was multiplied by 416 producing another range. For these latter intensities, only the smallest aperture could be used.

The second apparatus produced liminal stimuli by essentially the same principle, except that in addition to the intensity control by the current, an Aubert diaphragm was included. Thus the apparatus could be calibrated and then the whole level reduced tremendously. The range of this apparatus was from zero to 0.115 c/ft.^{2} Two observers were used, the author, and an individual untrained in psychological observation. The results in the two cases correspond adequately for the purpose of the experiments.

Results

The first demonstration that stray light is responsible for the flicker observed outside the test-object is produced by an arrangement similar to that used by Fry and Bartley (1) in studying retinal action potentials, namely, two equal sized, equally bright circles were intermittently presented on a dark field, the one disappearing just as the other appears. If there is entoptic scatter as postulated then this stimulus situation will produce not an intermittent scatter but a continuous illumination a distance away from the image. Consequently in a situation of this kind no general field flicker would be expected. Even with one cycle per second which is much below the c.f.f. at absolute threshold and the rate at which field flicker is very pronounced, the field remained steady, except adjacent to the circles where intermittent dispersed light existed to play a part. Were interaction to account for general field flicker, it would have occurred here, for at this slow rate of intermittency there was a marked flicker over the whole field when one of the two test-objects was screened off by a black card so that the field scatter was made intermittent.

The critical fusion frequency (c.f.f.) of the sensation aroused when the image of a test-object subtending a visual angle of 1 degree and ranging in intensity from .075 c/ft.^{2} to 7 c/ft.^{2} was made to fall within the optic disc was measured, and is shown in Fig. 1 (curve *a*). In this case we have a situation in which interaction *cannot* take place, there being no synaptic layer in the disc. If the frequencies here at all compare with any found elsewhere, they must be assigned to

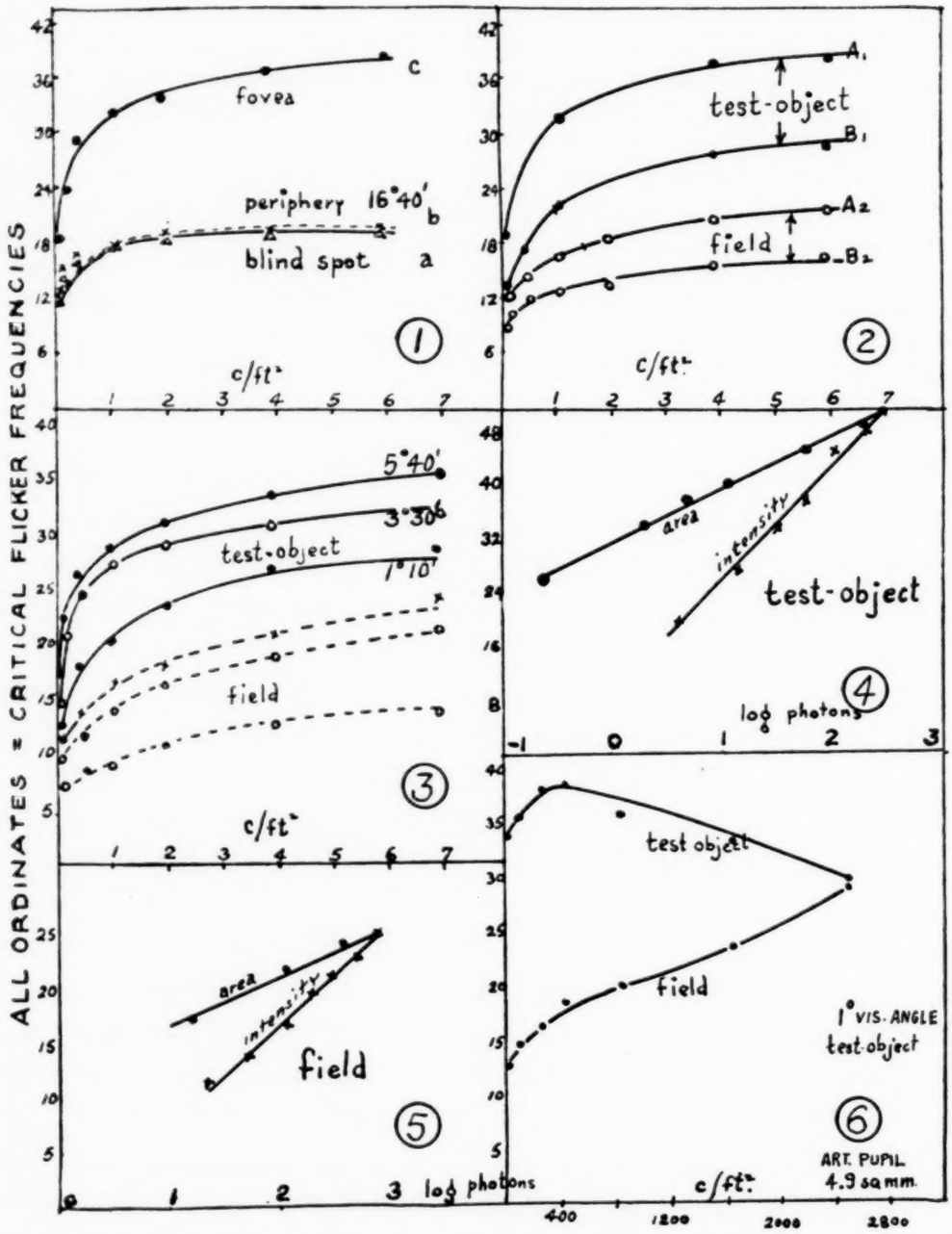

FIG. 1. Curves showing the relation between the critical flicker frequencies for images falling on fovea, blind spot and corresponding periphery.

FIG. 2. Curves showing the critical flicker frequencies for the test object and field adjacent to it when the object subtended a visual angle of 1 degree, having the range of intensities specified in the abscissa.

FIG. 3. A comparison of the critical flicker frequencies in test-object and surrounding field for different areas of stimulus.

FIGS. 4 and 5. A comparison of the effects of reducing are and intensity upon critical flicker frequency for the test-object and field respectively, in terms of retinal illumination (photons).

FIG. 6. A comparison of critical flicker frequencies for test-object and field at an intensity where they become identical.

the light scattered outside the disc. The assumption is that the disc is either not light sensitive or else very little sensitive,[3] for when an image is projected onto it, the absolute brilliance threshold obtained is much below that found in the retina equally peripheral. To compare with this, the image of the test-object was made to fall on the opposite side of the retina the same distance from the fovea. The results in this case are also to be found in Fig. 1 (curve *b*) which lies quite close to curve *a*. The virtual identicalness of the two curves in absolute value might not be expected. It arises in the following manner. When the measurements of the test-object in the periphery were being taken it was discovered that except at the lowest intensities we used, the flicker in it disappeared before that in the field immediately surrounding it, and so that as the intensities were increased it was the flicker of the field that was being measured. It then is not surprising that the c.f.f. at the two different points equally peripheral is identical. In the one case a sensitive area is receiving the image of the test-object and in the other, a virtually if not totally insensitive area is receiving it, notwithstanding, the c.f.f.'s in the field surrounding them are similar. The fact that the field flickers in these cases is to be assigned to stray light. It is conceivable however that some difference in the absolute value of the immediate field c.f.f. would be found in cases in which the image of the test-object was also flickering. This is to be tested by the experiments that immediately follow.

The first experiment determined the c.f.f. for a circular spot of light subtending a visual angle of 1 degree and on an unilluminated field. The intensity of the spot was varied in seven steps from 0.075 c/ft.^{2} to 7 c/ft.^{2} Under identical

[3] Some believe however that the optic disc has been or can be shown to be light sensitive (2) (3). In this connection it is relevant to state that when our light was directed upon the disc only a cloudy or diffuse patch was seen, while with the same light directed upon the corresponding point on the opposite side of the retina a definite bright disc corresponding to the test-object was seen and around it a cloudy halo such as appeared alone in the other case. This difference as well as the fact of the high sensation threshold for light falling on the optic disc shows that if it has any sensitivity at all it is very little.

conditions the c.f.f. of the field immediately surrounding the spot was also obtained. Typical results are shown in Fig. 2, and indicate that the c.f.f. of the latter is a constant fraction of the former. This is not to be expected if scattered light alone were determining the c.f.f. of the field, for the c.f.f. for all stimulus intensities should be such as to indicate that the intensities of the field were a constant function of those of the test-object. When the c.f.f.'s of the test-object and field themselves are constant in relationship this is not the case. It is to be supposed then that the field around the image is stimulated by stray light and immediately around the image by dispersed light also, and that especially in the field near the image border, there is some sort of interaction. It is to be noted that in this case it was this part of the field that was used.

Much lower c.f.f.'s result when the field remote from the image is measured, and with slow flicker in the test-object the whole field flickers. We know that the *existence* of flicker is dependent upon stimulation by stray light. If interaction is to have a part in determining the c.f.f. of the *whole* field, it must pass beyond the limitation set by Adrian and Matthew's experiments on the eel's eye. It is conceivable that there are more sensitive means of detecting interaction than they used, so that by the flicker method for example, an appreciable amount of it might be detected over the greater part of the visual field. This has not yet been determined, however.

The above experiments were repeated while using an artificial pupil 2.5 mm in diam. The curves *B-B* in Fig. 2 give the results. Their most outstanding feature is their general similarity to the curves for the naked eye, though naturally showing a reduction in c.f.f. Since our object was not to determine the relation between test-object intensity and flicker but to show the relation between flicker in the test-object and its surrounding field, the artificial pupil was used only occasionally throughout the course of the experiments such as in the comparison between the effects of area and intensity upon c.f.f. described below.

The area of the test-object was varied in the next experi-

ments, from 17 minutes to $10\frac{1}{6}$ degrees and the c.f.f.'s of both the test-object and the field were obtained for a range of intensities. A family of curves is shown in Fig. 3 for some of the areas. The solid curves are those for the test-object and the dotted ones are for the respective fields. Both sets show that as the area of the test-object is increased the c.f.f.'s increase. This experiment therefore does not appear to be a critical test of what is producing flicker in the field except that increase of *area* of the test-object is known to increase *intensity* of scattered light, and this should increase the c.f.f. of the field.

From the data at hand, and also from the data of a special series of measurements employing an artificial pupil, curves were constructed for equivalent reductions in area and intensity of the test-object to see whether these two variables behave alike in the control of c.f.f. Representative curves are shown in Figs. 4 and 5 for the test-object and field respectively. In neither case are area and intensity shown to act alike. Changes in area do not seem to affect c.f.f. as much as do those of intensity. It must be noted that the measurements of field fusion have to do with the part of the field adjacent to the image of the test-object where there are two determinants for the intensity of illumination, the dispersion in the media illuminating the area immediately surrounding the image which varies chiefly with intensity, and entoptic scatter over the retina in general which varies with both intensity and area. Further away from the image, entoptic scatter alone is the determinant of illumination and there the *area* and *intensity* of the test-object should act alike. Measurements of the c.f.f. at a distance away from the test-object are not reliable enough for the making of fine distinctions necessary in telling whether the area curve precisely matches the one for intensity. However, at or near the boundary between field and test-object, it would be expected that intensity of the test-object would control the c.f.f. of the field to a greater degree than would area.

It has already been mentioned that the test-object when used in the visual periphery was too bright to continue to

flicker when the c.f.f. for the field had been reached, except at the lowest intensities in our range. An analogous situation was encountered when a very bright test-object was used in the fovea. By increasing the intensity of the test-object a point was reached at which the c.f.f. did not increase with increasing intensity and with still higher intensities the c.f.f. even became lower. This same thing is reported by Hecht and Verrijp (4). But throughout this range the c.f.f. of the field was continually *increasing*. The result is that a point was reached at which the c.f.f. of the field and test-object were the same. This is shown in Fig. 6, the curves in which were obtained with the artificial pupil. With still more light upon the eye (possible with same stimulus when not using artificial pupil) the c.f.f. of the test-object became lower than that of the field. The curves for this are not presented. Entoptic scatter explains this result, one which would not be predicted by assuming interaction to be responsible for the *existance* of field flicker. It is to be noted however that the curve for the c.f.f. of the field which had begun to slope off to the horizontal again rises as the point at which the c.f.f.'s of the test-object and field become identical is reached. This instance along with the other curves showing the behavior of the c.f.f. of the field indicate that interaction of some sort *contributes* to determining the actual value of the c.f.f. at least near the test-object although it does not institute flicker itself.

The lowest c.f.f. for the test-object in the fovea ever obtained was 3.75 cycles per second. This is shown in Fig. 7 wherein an absolute threshold curve for brightness is graphed. The stimulus of course was intermittent so that for threshold for steady stimulation, intensities less than the bright phase of the cycle which is registered on the graph could be expected. With the conditions just mentioned the test-object would disappear at times while yet slightly flickering and at others fusion would occur just before it disappeared. It is significant to note that at no time have c.f.f.'s been obtained in the field with supraliminal test-objects which are lower than this threshold rate (3.75) though under one set of conditions this lower

rate was reached, namely when measuring the c.f.f. of the field 16°40′ away when the test-object was in the fovea and with the lowest intensity in the regular range (0.075 c/ft.^{2}).

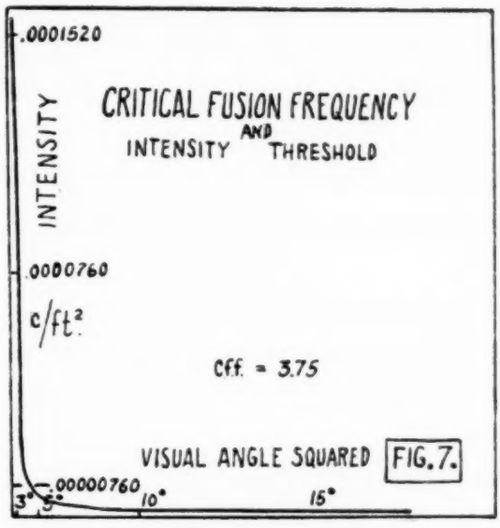

FIG. 7. A curve showing threshold for intensity and critical flicker frequency as stimulus area is varied.

This would seem to show that in most of our situations the entoptic scatter was *much* above threshold, if c.f.f. is to be any measure of it.

DISCUSSION

By photometry of the external globe of the freshly excised eye, by registration of cortical action currents from light admitted through the sclera, and by comparison of the threshold of response at the cortex for certain stimulus patterns the functioning of stray light has been demonstrated (**5**) (**6**). It has also been found to produce the retinograms recorded by Fry and Bartley (**1**) and it explains the manipulation of the differential limen for brilliance at the fovea by various amounts of light in the peripheral field (**7**) and also the manipulation of visual acuity under certain conditions (**8**). Measurement of the latency of the response at the cortex revealed its contribution in relation to the image (**6**). In this connection it is interesting to note that the existence of light not even seen is demonstrated by the nerve action potential technique, and in the neurology of vision it may at times play a larger part than that which is seen (the image).

Summary

1. Two demonstrations that flicker in the field outside of the test-object is instituted by stray light rather than by retinal interaction are described.

2. The critical fusion frequencies of the field in relation to those of the test-object indicate that after flicker is instituted by stray light, some sort of interaction between the two areas of the retina stimulated at different intensities helps to determine critical fusion frequency of the field at least near the test-object.

3. The flicker method has afforded an additional means of demonstrating the functional existence of light that under other conditions is not seen.

(Manuscript received February 1, 1935)

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THE EFFECT OF STEADY STIMULATION OF ONE PART OF THE RETINA UPON THE CRITICAL FLICKER FREQUENCY IN ANOTHER

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Lythgoe and Tansley [1] investigated the effect of varying the brightness of field surrounding a patch used for testing critical flicker frequency (hereafter c.f.f.), and found in general that the c.f.f. increased as the brightness of the surrounding field increased, but they carried the brightness of the surrounding field only up to the level of the Talbot brightness of the test spot.

Graham and Granit [2] found, as Lythgoe and Tansley had found, that a steady stimulus equal to the Talbot brightness of the intermittent stimulus gives a higher c.f.f. than the intermittent stimulus alone. They alleged that this is evidence of a "true spatial summation which implies that a higher state of excitation may be produced in a given area through the agency of impulses from any area reasonably near to it." Now flicker so far as the retina is concerned involves an alternation of strong and weak phases of activity, and it is the difference between the phases and not their absolute strength that counts. Even though summation might make the strong phases stronger, as Graham and Granit suppose, it ought to make the weak phases stronger also, so that it is difficult to see how summation could raise the c.f.f.

When the brightness of the steady stimulus was made brighter than the Talbot brightness of the intermittent

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[1] Lythgoe, R. J. and Tansley, K., The adaptation of the eye: its relation to the critical frequency of flicker, *Med. Research Council Rep.* (*Great Britain*), 1929, No. 134.

[2] Graham, C. H. and Granit, R., Comparative studies on the peripheral and central retina, VI. *Amer. J. Physiol.*, 1931, **98**, 664–73.

stimulus, the c.f.f. was lower than when the two brightnesses were equal, and under certain conditions was actually lower than when the intermittent stimulus alone was used. Graham and Granit regarded this as indicative of the presence of a second factor which they called *inhibition*.[3] They regarded the inhibitory mechanism to be the same as that which operates in ordinary brightness contrast phenomena. They assumed that this factor does not come into play until the brightness of the steady stimulus reaches a critical value somewhat above the Talbot brightness of the intermittent stimulus. They offered no concrete explanation of how the inhibitory mechanism lowers the c.f.f.

It seems to us that the *depression* [3] mechanism is adequate to account for all the pertinent facts without the aid of summation. Now when the steady stimulus is less than or equal to the Talbot brightness of the intermittent stimulus it is conceivable that the weak phases might be depressed whereas the strong phases might remain unaffected. This would suffice to raise the c.f.f. above the value which obtains when the steady field is absent. When the steady field is raised somewhat above the Talbot brightness of the intermittent stimulus, the strong phases might also begin to be depressed and thus the c.f.f. would fall.

In trying to account for the facts in terms of a depression mechanism without the aid of summation, we do not mean to imply that the retina is incapable of summation. Its failure to manifest itself might be due to the fact that the range of summation is too limited for it to play a predominating role in the phenomena under question. Summation might occur through (1) the convergence of photoreceptors on bipolar

[3] Graham and Granit have suggested that the inhibition effect is brought about by inhibitory paths in the retina, which they point out to be the amacrine cells. Presumably they suppose that the excitation of these cells causes them to secrete a substance which depresses nervous activity. Although this hypothesis is attractive to those who are interested in applying the concepts of synaptic functions in the spinal cord to the retina, the theory as so far developed does not account for detailed visual phenomena and too much hope ought not to be entertained for its ultimate success. Inasmuch as the word *inhibition* implies the above specific theory of nervous functioning, we are proposing to use the term *depression* which is noncommittal as to the nature of the physiological processes involved.

cells, (2) the convergence of bipolar cells on ganglion cells, and (3) the convergence of internuncial neurones on a common focus. Beitel [4] has pointed out that convergence of the first two types might provide for summation over a range of 20′ visual angle in the periphery, but it is practically absent in the fovea. If the internuncial cells serve an inhibitory function and are not available for summation, the range of summation would not be adequate to account for the effect of a steady area upon an intermittent area, for the effect may involve a visual angle of 5°. There is no final proof, however, that the internuncial neurones are not available for summation effects. Furthermore, since, at the c.f.f., activity is still intermittent at the point where the paths from the two eyes converge,[5] the interaction effects with which we are concerned in the present paper might be seated at a higher level than the retina where little is known about the structural limitations of the process involved.

Geldard [6] has described phenomena which he alleges to be an anatomical isolation of separate facilitation and depression factors. When he confined both stimuli to the fovea, using two semi-disks separated by a narrow line, one for steady stimulation and the other for testing c.f.f., he found that the c.f.f. increased with the intensity of the steady stimulus up to a point above the Talbot brightness of the test field and then it began to decrease. This agrees with what the previous investigators had found. When a spot 5° out in the periphery was stimulated, the c.f.f. at the fovea continued to rise as long as the intensity of the peripheral stimulus was increased. Geldard attributed the reversal with the foveal stimulus and its absence with the peripheral stimulus to the fact that in the fovea a depression mechanism is brought into play in addition to a facilitation mechanism already operating but this de-

[4] Beitel, R. J., Jr., Spatial summation of subliminal stimuli in the retina of the human eye, *J. Gen. Psych.*, 1934, **10**, 311–27.

[5] Sherrington has shown that the c.f.f. can be affected by varying the phase relations of stimulation of corresponding areas of the two retinae.

[6] Geldard, F. A., Foveal sensitivity as influenced by peripheral stimulation, *J. Gen. Psych.*, 1932, **7**, 185–89; Flicker relations within the fovea, *J. Optical Soc. Amer.* 1934, **24**, 299–302.

pressive mechanism is absent in the case of the peripheral stimulus.

The experiments to be described below show that the critical point for the reversal varies with the distance of the steady field from the intermittent field, and whether it occurs within a given range of intensities depends upon what particular distance is used. If the distance is great as in one of Geldard's experiments, the reversal may not occur. Thus Geldard's facts do not necessarily prove that summation alone is present in the periphery. Depression alone can account for the facts in accordance with the principles outlined above, provided the additional principle is added that the critical brightness for the depression of the bright phases varies with the distance between the two stimuli.

We have used three stimulus patterns, each of which consisted of a disk which served as the test field, and an annulus surrounding it which served as the activating stimulus. In one experiment the activating field and the test field were strictly contiguous; in another the interval between them was 1/8 in and in the third experiment the interval was 5/16 in. The diameter of the outside of the annulus was 1¾ in in all cases. The central disk was 3/8 in in diameter in the first two experiments and .44 in in the last. The distance to the eye was 33.5 in. The left eye was shielded completely, and an artificial pupil 2.33 mm in diameter was placed before the right. The Talbot brightness of the disk was .5 c per sq ft and its c.f.f. was tested for different intensities of the annulus. The results are given in the graphs in Fig. 1. The readings for each graph were all taken at one sitting, five readings were taken for each brightness level of the annulus and then the experiment was repeated. The dots represent the averages for the ascending order, and the crosses the descending order. The circles represent averages for the two series.

As the distance between the test field and the annulus increases the reversal in the curve occurs later on the intensity scale and does not appear within the range of intensities used when the interval is as much as 5/16 in. These results suggest

that Geldard's failure to obtain a reversal with an activating stimulus 5° away from the test field, resulted from his failure to use high enough intensity, but since he used a stimulus having a brightness of 369 c per sq ft, it is doubtful whether with a separation of 5° the stimulus can be made bright enough to produce the effect.

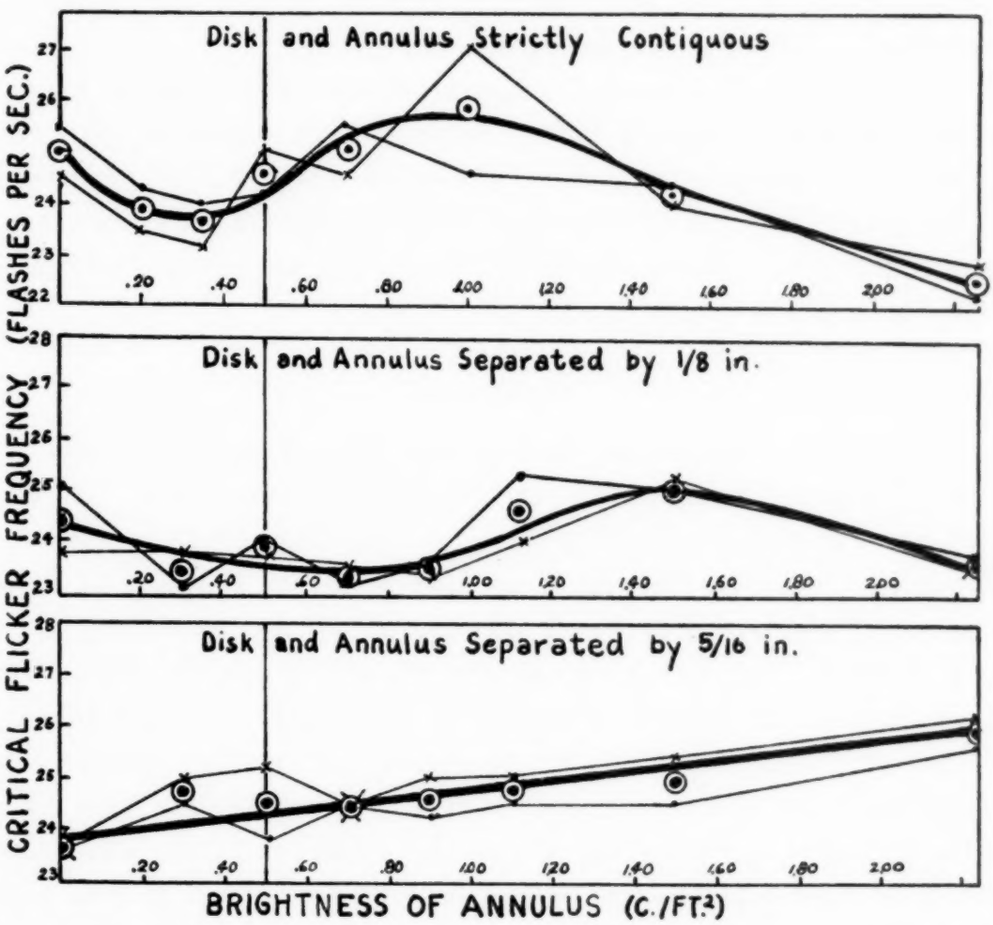
Disk and Annulus Strictly Contiguous
Disk and Annulus Separated by 1/8 in.
Disk and Annulus Separated by 5/16 in.
CRITICAL FLICKER FREQUENCY (FLASHES PER SEC.)
BRIGHTNESS OF ANNULUS (C./FT.²)

FIG. 1. The effect of varying the brightness of an annulus surrounding a disk-shaped test field upon critical flicker frequency. The three graphs show the effects obtained with different degrees of separation of the disk and annulus. In each graph the Talbot brightness of the disk is indicated by the vertical line. The points represent the values obtained when the readings were made in the ascending order of brightness of the annulus; the crosses in the descending order; and the circles the average values.

We have not found a good explanation for the initial drop in the curves which is quite pronounced in the case of the upper graph, and was obtained for both orders in which the readings were taken. Lythgoe and Tansley [7] obtained a

[7] Lythgoe, R. J., and Tansley, K., *Op. cit.*

similar result when they used peripheral vision instead of the fovea and attributed the result to the diverse roles played by the rods and cones, but this explanation would not apply in our case where the fovea was used, and where the test field subtended a visual angle of only 45′. Furthermore their explanation involves the questionable assumption that the stimulation of one part of the retina affects the state of adaptation of neighboring parts.

Graham and Granit [8] compared the effect of a foveal steady stimulus upon a foveal intermittent stimulus with the effect of a peripheral steady stimulus upon an intermittent peripheral stimulus and alleged that inhibition was absent in the periphery. But their conditions were too limited to make their interpretation reliable.

Summary

A steady stimulus applied to one area of the retina affects the c.f.f. of an intermittent stimulus in another area. As the intensity of the steady stimulus is increased from zero upward, the c.f.f. rises, but when a critical point is reached it falls again. This fact has been accounted for by assuming that the steadily stimulated area depresses activity in the flickering area; below the critical intensity it depresses the weak phases of activity and raises the c.f.f.; above the critical intensity it depresses the strong phases also and thus lowers the c.f.f. The fact that the reversal of the effect fails to occur when the distance between the two stimuli is large, is due to the fact that the critical intensity increases with the distance and when the distance is large the critical intensity lies beyond the range of intensities investigated.

Much is owed to Drs. G. H. Bishop and P. W. Cobb for criticisms and suggestions.

(Manuscript received March 18, 1935)

[8] Graham, C. H., and Granit, R., *Op. cit.*

REACTION TIME BEHAVIOR AFTER CAFFEINE AND COFFEE CONSUMPTION

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Parallel with the world increase in the consumption of caffeine beverages during recent years, there has been a series of scientific studies published upon the subject of caffeine effects in man. These investigations have been concerned primarily with the action of caffeine in the capsulated form as compared with an equivalent amount in the coffee beverage. A few articles have dealt with the effects of decaffeinated coffee.

A study of the caffeine and coffee literature with reference to reaction time reveals an unfortunate extension of the experimental data over a period of several months in order to obtain statistical detail. The recent paper by Horst and Jenkins (1) dealing with reaction time, states that their data covers a period of fourteen weeks in the case of some of their male subjects. Such prolonged experiments produce data which confuse the interpretation of immediate or general behavior of the organism as influenced by a standardized dosage of caffeine in the pure or beverage form. Statistics derived from the latter period of observations made continuously for several months, may be misleading because the human organism develops tolerance to the caffeine. Myers (2) noted this tolerance to caffeine in 1924 in his studies of renal secretions. An excellent paper by Winsor and Strongin (3) published in the JOURNAL OF EXPERIMENTAL PSYCHOLOGY in 1933, demonstrated the development of tolerance to caffeine within six weeks with reference to parotid gland secretions. Coincident with tolerance, the response of the body to caffeine is altered definitely and often to a high degree. Variation in the dosage of caffeine disturbs the tolerance level drastically

and a new and different level will be established within limits. Tolerance is developed for a given dosage, but complete tolerance is not obtained. Habituated subjects respond to a standardized dosage of caffeine in the pure or coffee beverage form, but to a smaller degree and for a shorter duration. Obviously, investigations to determine the effect of caffeine or a caffeinated beverage must control these factors of the length of the experimental period and the original habituation of each subject to caffeine consumption. Individual variations, due to other more intangible factors, exist with regard to the degree and duration of the response due to caffeine stimulation and may become disturbing factors in the interpretation of the results. Horst and Jenkins (1) also noted a definite age variation in reaction time behavior as influenced by caffeine.

In order to correct for such errors and to eliminate or to reduce such factors to a minimum in the reaction time data presented here, five subjects were selected from an extensive experimental series conducted with normal young women of college age. It proved to be difficult to find individuals whose normal behavior was equivalent in as many factors as seemed to be mandatory in this study. The five subjects reported in this paper, however, possessed *equal normal* (established normal) *reaction time*, *equal age*, *height* and *weight*. Only by such an approximation of factors would the data derived be comparable in terms of equal dosage per kilogram of body weight and also with reference to the great similarity in the individual variations. The *subjects* were *habituated to one cup* of coffee per day prior to the experimental period and the *investigation in no case* involved *more than four weeks*. By observance of these controls, behavior variations in the reaction times in comparison with the subjects established normal reaction time, could be assigned to the capsulated caffeine treatment or to its equivalent in the coffee beverage. On such a basis, the five subjects really present a composite case of unusual significance; or, at least five times as significant as the record of any one instance chosen at random.

The method of measuring the reaction times in man was

found to be satisfactory by the use of a simplified modification of Dr. Metfessel's psychodometer which was described by me (**4**) in 1934. This apparatus registers the results of the psychophysiologic test employed in time intervals of one one-hundred twentieth of a second. The stimulus was given by the use of a number of small colored electric bulbs in circuit with an impulse counter. The lighted bulb was turned out and the counter stopped by pressing the proper tap-key. The details of the recording procedure and controls have been described previously (**5**) and consists primarily of requiring each subject to refrain from the use of caffeine beverages and nicotine for at least twenty-four hours prior to test days. In addition, a constant number of hours of sleep per night was maintained before experimental days, and a constant breakfast menu was prescribed for all test days. A normal preliminary day test preceded each experimental series and a check control test was recorded on the day following each series. Thus, the reaction time of each subject was studied under normal, caffeine-treated, coffee-treated, and capsulated starch control conditions. The total reaction time included any lags in the apparatus and involved five psychophysiologic phenomena from the original stimulus of the subject's eye followed by sensory nervous conduction to the brain, transmission from sensory to motor centers to the muscles of the hand with the resulting coördinated movement determined by the mental decision within the brain in accordance with the color of the light flashed. On all experimental days, a set of 25 readings was made at each test, and from one to four sets were recorded. A separate test was made at half-hour intervals from zero to 180 minutes and followed in each case by a twenty-four hour test.

On non-treated days, 300 c.c. of hot water was drunk prior to the tests. On caffeine days, a three-grain capsule (alkaloid, Merck) was given with 300 c.c. water; and, on the coffee days, 300 c.c. of hot black coffee was consumed. This coffee was prepared from freshly-roasted beans of a constant pulverized grind and by a timed method of extraction which resulted in a caffeine content of three grains per 300 c.c. of

the beverage as analyzed. On check-control days, a starch capsule was taken with 300 c.c. of water.

It was found essential to submit each subject to a practice period in order to correct for the difference between the original normal reaction time and the normal reaction time after several days' performance. Each reaction time interval was determined as an average for five equal subjects. For example, the *first interval* cited for the first day is 88/120th second, and is derived as follows:

Subject No. 1, first reading on *her* first day was 90/120th second
Subject No. 2, first reading on *her* first day was 86/120th second
Subject No. 3, first reading on *her* first day was 88/120th second
Subject No. 4, first reading on *her* first day was 89/120th second
Subject No. 5, first reading on *her* first day was 88/120th second

The *average* reading on the *first day* of five equal subjects, each of whom reached an equal established normal on the third day, was 88/120th second. It will be noted from Table I, which shows the average behavior of the composite of five subjects' records on ten successive days, that a reduction in normal reaction time occurred to the extent of 0.14 second. The final normal reaction time level is referred to as the *established normal* which for these subjects proved to be 0.50 second for the test employed. Figure 1 indicates clearly that the subjects developed an established normal reaction time, for the relatively simple test involved, on the third day from which their subsequent variations on the fourth to tenth day inclusive were not significant as shown by the superimposed nature of the curves for the third to seventh day inclusive on Fig. 1.

It should be noted also that the extent of the variations between individual readings is reduced definitely with practice in the development of the established normal. The

TABLE I

DETERMINATION OF ESTABLISHED *Normal* REACTION TIME

Each figure = the average of five female subjects for the given reaction.*

Trial No.	1st Day	2nd Day	3rd Day	4th Day	5th Day	6th Day	7th Day	8th Day	9th Day	10th Day
1	88/120	77	63	64	68	60	62	65	67	65
2	77	79	61	63	57	58	58	65	69	60
3	73	70	57	68	60	56	62	60	70	66
4	77	69	59	65	65	60	62	63	69	63
5	80	71	60	65	57	57	59	68	69	60
6	74	68	58	63	60	58	60	65	53	57
7	76	80	61	62	64	60	61	57	58	55
8	78	69	60	61	55	65	58	60	63	60
9	80	77	59	63	62	68	60	65	59	57
10	68	75	62	63	60	61	59	64	65	62
11	74	71	62	66	60	60	57	60	59	65
12	75	74	58	56	64	54	61	63	64	63
13	75	73	62	58	55	62	63	65	60	65
14	75	70	55	62	60	56	58	64	61	61
15	75	71	59	59	57	62	60	60	57	58
16	76	75	62	65	59	59	58	55	63	55
17	80	69	72	57	59	57	61	60	65	57
18	72	74	65	65	65	58	58	59	64	60
19	72	69	64	59	63	55	64	63	70	62
20	76	64	65	70	65	57	65	62	58	63
21	80	68	62	65	60	54	65	60	62	63
22	84	63	55	60	64	59	62	60	55	65
23	80	66	61	62	63	57	69	63	64	59
24	79	67	58	61	65	61	62	60	56	60
25	83	62	60	65	63	63	59	57	60	60
Total	1927	1771	1520	1567	1530	1477	1523	1543	1560	1521
Ave.	77.08	70	60	62	61	59	60	61	62	60
	120	120	120	120	120	120	120	120	120	120
Ave. R.T. (sec.)	0.64†	0.58	0.50†	0.51	0.50	0.49	0.50	0.50	0.51	0.50

* Each point = average reaction time of five female subjects of approximately equal age, weight, and established normal reaction time.

† In these subjects, practice reduced an *original* normal of 0.64 second to an *established* normal of 0.50 second.

mandatory necessity of determining the established normal reaction time prior to experimental treatment days in these studies is obvious. Unfortunately, the determination of such an established normal for each subject in accordance with the subject's caffeine habituation has not been recorded by some investigators with sufficient precision prior to caffeine and coffee experimental days. This accounts for some of the divergence of data reported by various authors and has led partly to the confusion apparent in the conclusions drawn. My own recent paper (**5**) and the paper by Horst and Jenkins

(1) show considerable individual variation in the subjects' reaction time response and also the presence of a dosage effect. Bellis (6) noted the age factor in relation to reaction time response in man and Horst and Jenkins (1) demonstrated recently the variation in the reaction time in men in the third, sixth, seventh, and eighth decades of life.

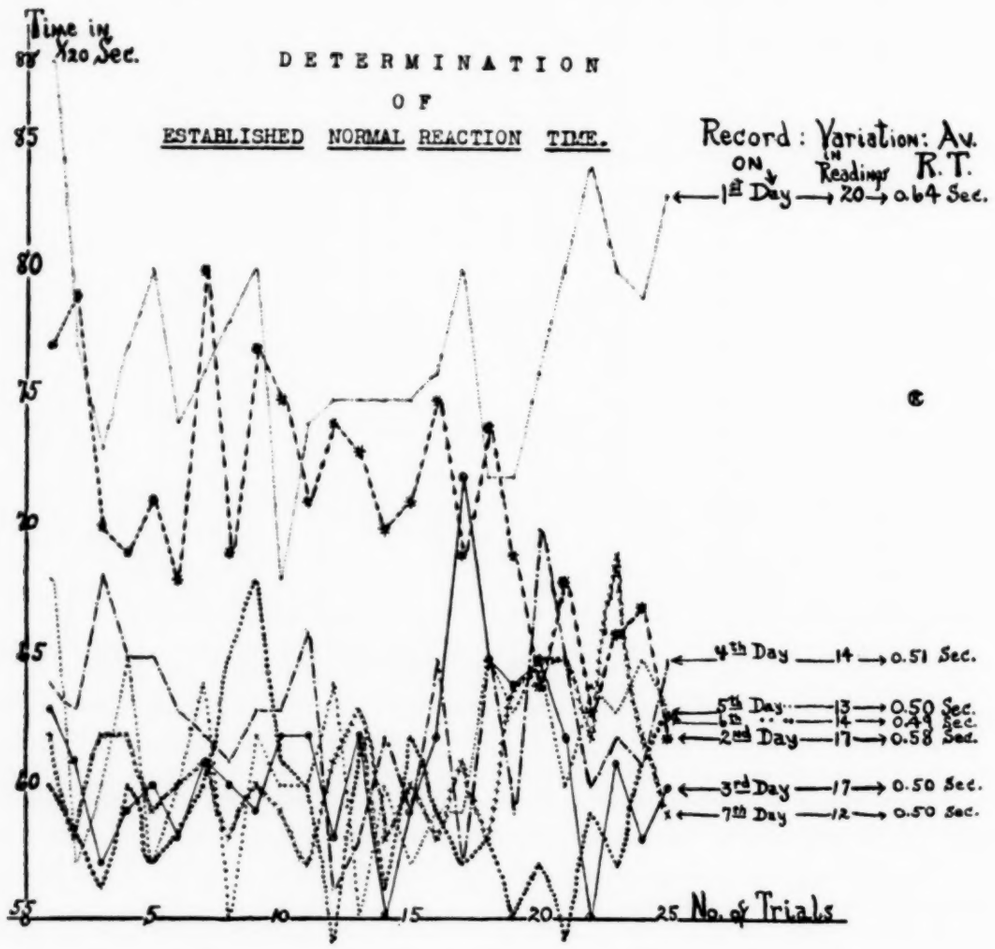
Time in X20 Sec.
DETERMINATION
OF
ESTABLISHED NORMAL REACTION TIME.
Record : Variation : Av.
ON Readings R.T.
1st Day — 20 — 0.64 Sec.
4th Day — 14 — 0.51 Sec.
5th Day — 13 — 0.50 Sec.
1st " " — 14 — 0.49 Sec.
2nd Day — 17 — 0.58 Sec.
3rd Day — 17 — 0.50 Sec.
7th Day — 12 — 0.50 Sec.
No. of Trials

FIG. 1.

Each of the foregoing factors, if not considered in the interpretation of results, are sufficient within themselves to more than account for the reported effects of caffeine and coffee. In view of this fact, special attention has been given to the choice of subjects for the results reported here. The five subjects were of the *same* sex (college girls), approximately of the *same age*, *weight*, *height*, and *established normal reaction time*. The comparison of the data derived from these

unusually equivalent subjects when subjected to the same dosage (3.3–3.6 mgs/kgm body weight) has been plotted in Fig. 2 for comparison of their reaction times, over a three-hour period with a twenty-four hour control reading, with relation to their caffeine and coffee reaction time as compared to their established normal reaction time. Each point represents the average of the average of 25 readings of the five subjects. The data for the points on the curve were derived from the statistical work sheets which are omitted here for brevity.

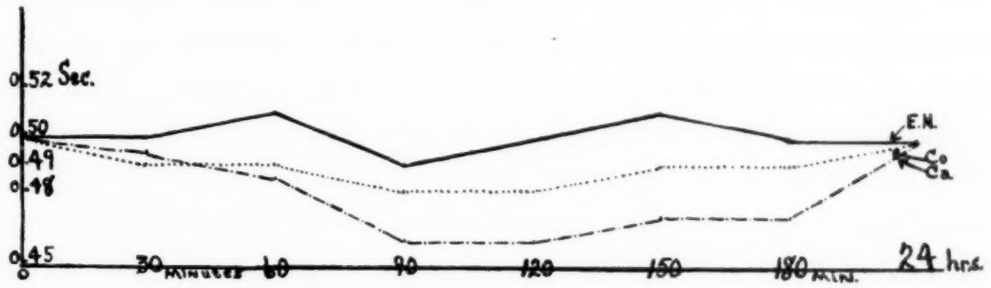

FIG. 2. Reaction time. Effect of caffeine and equivalent amount in coffee on *established* normal R.T. in normal young women.* *Each pt.* represents the *average of the average* of 25 readings of 5 subjects having approximately equal established normal R.T. (0.50 sec.); age (20–22 yrs.); height (162–165cm); weight (55–60 kgms). Dosage: 3.3 to 3.6 mgms/kgms body wt. *or* 0.05 to 0.054 grain/kgm.

——— = Established normal after 300 cc water.
. . . . . = Black coffee (300 cc).
.—.—.— = Caffeine (alk., Merck) 3 grain capsule with 300 cc water.

Note: Maximum decrease from E.N. after coffee = 0.02 sec. = 4 percent. Maximum decrease from E.N. after caffeine = 0.04 sec. = 8 percent.

It will be noted that coffee is slightly more effective than caffeine within the first thirty-minute period. Between the thirty and sixty-minute period after consumption the caffeine curve crosses the coffee curve and remains below throughout the experiment. The maximum decrease in reaction time from the established normal after coffee was 0.02 second, or the equivalent of 4 percent for these subjects; whereas, the maximum decrease after the pure caffeine was 0.04 second or 8 percent. These percentages for young women are somewhat higher than the 2 percent to 6 percent decrease in reaction time reported by Horst and Jenkins (1) for man sub-

* Coffee habits: One cup per day (av.). Nicotine habits: Cigarette occasionally.

jected to similar dosages. The sex and age factors may account for this difference. Horst and Willson (7) reported in April 1935, that women were twice as susceptible to coffee as men.

Fig. 2 clearly demonstrates the fact that both caffeine and coffee reduce the reaction time of standardized subjects under a standardized caffeine dosage of 3.3 to 3.6 mgms/kgm body weight throughout a three-hour period. This fact is in agreement with Horst and Jenkins (1) and in disagreement with Schilling (8), who reported an increase in reaction time under the influence of caffeine or coffee. It has been reported by Hare (9) in 1925 that caffeine beverages accelerate the perception of sensory impressions and quicken thought. Both of these factors would tend to reduce reaction time. Fig. 2 also indicates definitely that caffeine in the form of the coffee beverage is significantly less effective in reducing the reaction time than an equivalent amount in the capsulated form of the pure alkaloid. This finding is in contradiction to the report of Lehmann and Weil (10) that no essential difference occurs in the responses of man to coffee, tea and caffeine when the caffeine is administered in equivalent doses. This difference between the effect of the pure caffeine and the caffeinated coffee beverage is emphasized with reference to the established normal and to its two forms of consumption by the 120-minute readings as listed in Table II. The 120-minute period after consumption represents the time at which both caffeine and coffee produced a maximum effect for these subjects under the dosage administered and in cognizance of the fact that these subjects were habituated to a dosage equivalent to one cup per day. Figure 3 illustrates the fact that capsulated caffeine is more effective than coffee in reducing the reaction time at the maximum effective period for both. Table II and Fig. 3 also indicate that the maximum variation in individual readings of these subjects at the 120-minute period was 14/120th second in both the established normal and the coffee tests. In the caffeine record at the same period the maximum variation was somewhat less; namely, 12/120th second. Whatever effects were produced by either caffeine

or coffee on a test day were not apparent during the check-control series at the twenty-four hour period.

TABLE II

ESTABLISHED NORMAL VS. COFFEE VS. CAFFEINE

Each figure * represents the average of 25 readings for five young women. At the 120-Minute Period. Reaction Time in 1/120 Second.

Trial Number	Established Normal Readings	Coffee Readings	Caffeine Readings
1	62	58	60
2	58	62	60
3	59	59	50
4	62	55	54
5	69	51	60
6	62	61	53
7	62	57	50
8	59	53	56
9	63	56	60
10	61	54	60
11	55	54	54
12	60	65	55
13	59	61	53
14	62	61	60
15	65	54	50
16	66	56	56
17	64	54	57
18	57	60	60
19	61	57	62
20	63	51	55
21	58	55	60
22	60	59	60
23	58	58	60
24	61	63	59
25	58	58	55
Total	1524	1444	1409
Average	60/120	58/120	56/120
Ave. R.T.	0.50 sec.	0.48 sec.	0.46 sec.
Maximum variation	14/120	14/120	12/120

* Each figure is the average of five corresponding figures—one for each subject.

The effect of caffeine upon the speed and quality of performance in typewriting was reported by Hollingworth (11) in 1912. He concluded that the speed of performance was quickened by caffeine in one to three grain dosages and retarded by four to six grain dosages. He measured the quality of performance by the number of errors and found fewer errors with caffeine in all dosages administered (1 to 6 grains). Hollingworth reported increased speed and decreased errors present simultaneously.

More recently this *speed* factor, which is a reaction time value, has been studied by Reiman (**12**) with reference to the influence of coffee on the association constant. The conclusions from Reiman's report were to the effect that the power to form associative bonds was increased by coffee; and, that the facilitation in writing combinations learned was improved by coffee as shown by the *increase* in value of the *speed factor* on coffee days.

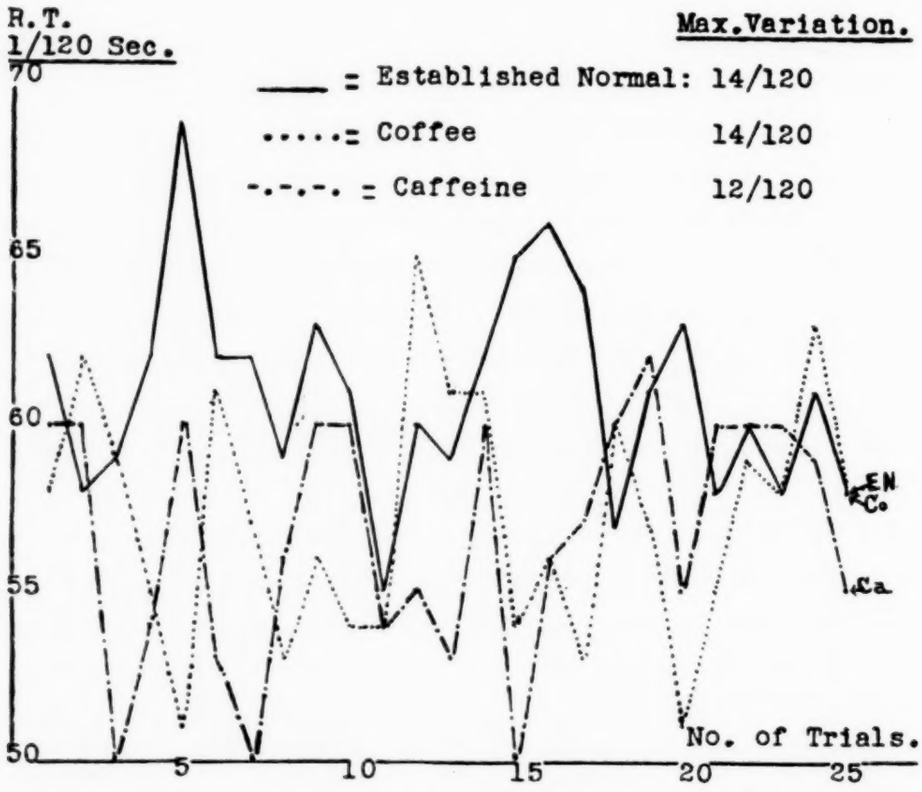

FIG. 3. Reaction time. Established normal vs. coffee vs. caffeine at the *120-minute period*. Each pt. represents the Average of 5 corresponding readings for each of 5 subjects.

With an abundance of accuracy statistics available from data on my reaction time work sheets, I checked my records for the number of errors noted during each trial-set of twenty-five readings. This sort of psychophysiologic performance offered an opportunity for errors in view of the fact that the subject was obliged to determine the color of the light signal flashed, coördinate his mental decision with his direction of hand movement for effective action in reaching for, and

pressing down, the corresponding tap-key in order to stop the impulse counter. An error was interpreted as the pressing of the wrong key or of clumsily fumbling on the proper key indirectly.

The total number of errors is plotted on Fig. 4 for the five subjects. There was a high degree of accuracy in some sets of twenty-five trials as the test was a simple one. However, the record of errors refers only to the performance *after* the established normal had been determined. Sufficient individual trials for each subject were recorded to render some significance to the data derived from the total analysis. The time required for these experiments was a minimum of three

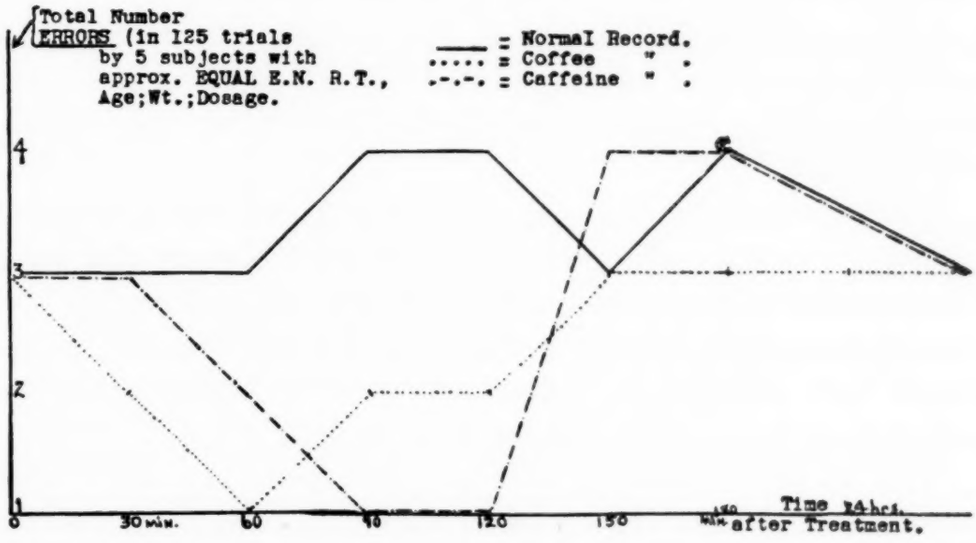

FIG. 4. Accuracy graph (After determination of established normal).

hours on three days with a minimum of 25 trial readings at each of seven periods by five subjects. The number of trials of established normal check days, caffeine and coffee days, and starch check-control days (omitted in Fig. 4 as only one time period was recorded on these days) totalled several thousands, one-third of which were established normals, one-third caffeine trials, and one-third coffee trials. The results are plotted on Fig. 4. It is obvious that the number of errors was reduced by a 3.3 to 3.6 mgms/kgm body weight dosage, both by caffeine and coffee for a two-hour period after consumption. The maximum accuracy increase after coffee occurred at the

one hour period, but persisted to some extent throughout the three hour period. After caffeine, however, the maximum accuracy increase was at the one and a half to two hours period which was followed by a relatively great increase in errors in the next thirty minutes in comparison with the maximum decrease in errors. However, the greatest number of errors during the three hour period did not exceed the maximum errors shown by the established normal curve. No twenty-four hour effect was apparent.

Summary

The development of the established normal reaction time occurs on the third day for the type of test employed. Therefore, at least a preliminary three-day series is mandatory prior to the acceptance of any experimental data as being indicative of a significant variation in the behavior from the normal reaction time.

Both caffeine (3.3 to 3.6 mgms/kgm) and the equivalent quantity in black coffee reduce the reaction time to the maximum extent of 8 percent and 4 percent respectively, in the case of the young women subjects in the performance of the simple test employed. Some reduction in reaction time is maintained for at least a three hour period.

The caffeine alkaloid is more effective than the coffee beverage except during the first half hour when the reverse order was observed. The most effective period was one and one-half to two hours after treatment. At the average maximum effective period of 120 minutes the individual variations between readings was approximately equal on the established normal control day in comparison with the coffee-treated day. Pure caffeine produces slightly less variation between individual readings.

The human organism responded selectively to the pure caffeine in contrast with the alkaloid in the coffee beverage with reference to the accuracy of the performance of the test. Coffee produced an increased accuracy which varied only in degree, during the entire three hour period; whereas, the capsulated caffeine favored increased accuracy only during the

first two hours of the experimental period. The final hour recorded a decreased accuracy equivalent to, but not exceeding, the established normal accuracy. Twenty-four hour effects from the dosage administered were not apparent.

(Manuscript received May 7, 1935)

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ON THE ELECTRICAL EXCITABILITY OF THE AUDITORY ORGAN ON THE EFFECT OF ALTERNATING CURRENTS ON THE NORMAL AUDITORY APPARATUS

BY G. V. GERSUNI AND A. A. VOLOKHOV

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The experimental study of the functions of the auditory nerve tracts may proceed in different ways. The investigation of the electric phenomena which arise at the activity of the nerve-elements of the auditory apparatus may be regarded as a method for such analysis. Forbes, Miller and O'Connor (1927) and Weyer and Bray (1930) were the first to carry out such investigations on warmblooded animals.

In order to study the functional properties of the nerve structures of the human auditory apparatus, it was also possible to proceed in another way, by adopting the method of electrical stimulation. The latter method often used when studying the other sense organs (*e.g.* the eye, the vestibular apparatus) has been altogether neglected with regard to the organ of hearing, in spite of Brenner's well known investigations (1868) on the electrical stimulation of the auditory apparatus.

While analysing the phenomena observed during the flow of direct current or during single current impulses (condenser discharges) through an unaffected auditory apparatus Gersuni, Lebedinski and Volokhov (1934) came to the conclusion that the arising auditory sensations were the result of the direct stimulation of the auditory nerve-fibres or terminals. These conclusions were confirmed by more detailed investigations performed on people whose middle and inner ears were functionally excluded (Andreev, Gersuni and Volokhov 1934).

The obtained quantitative characteristics of excitable tissues (chronaxie, strength duration curve) could have been

thus referred to the fibres or to the filaments of the auditory nerve. The magnitudes of chronaxie, which are very proximate to the magnitudes obtained during stimulations of other afferent nerve fibres, have made it possible to maintain, that there was no reason to ascribe special properties to the nerve tracts of the auditory apparatus, as some authors do (see Boring 1926).

The phenomena arising under the effect of alternating currents proved to be more complicated viz.: it was discovered that during the flow of alternating currents through uninjured human ear auditory sensations arose, very much resembling in character those, observed during adequate stimulation (Gersuni, Lebedinski and Volokhov). These sensations arising during electrical stimulation bear the character of a definite pitch and always correspond to the frequency of the electric vibrations.

Several explanations of these phenomena could be suggested. One of them consisted in the admittance that in some portions of the alternating current circuit mechanical vibrations arise, which stimulate the auditory apparatus in the ordinary way. Another explanation admitted the direct stimulation of the nerve structures of the auditory apparatus by the alternating current.

The present study deals with the experimental solution of these problems.

Method.—The chief features of the method applied consisted in the following: Alternating currents of various frequencies were conducted to the ear; simultaneously observations of the arising auditory sensations were carried on. The experiments were performed on people (5 observers) with normal hearing. A liquid (usually a normal saline solution) was poured into the external auditory canal. The active electrode, consisting of a thin silver wire in a rubber tube with a diameter of 2 mm was placed near the ear-drum. The projecting rims of the rubber tube prevented the direct contact of the electrode with the walls of the canal. A silver plate of an area of 15 sq. sm. covered with a piece of gauze and soaked in the saline solution served as inactive electrode. The plate was fastened to the back of the forearm.

A tube-oscillator for sound frequencies working on self excitation served as a source of electric vibrations from 750 to 17,000 d.v. In order to exclude the constant component, a stepdown transformer was connected with the anode-circuit of the amplifying tube. The active tension was connected with the body circuit in a potentiometric way. The strength of the alternating current through the body was obtained by means of a vacuum thermocouple. Thus currents of 0.05–5 m.A. could be measured with an accurans no less than 3 percent (Fig. 1).

The alternating tension, connected with the body-circuit, was measured by means of a tube voltmeter. The measurements of the body-circuit resistance were made for the alternating current (1000 d.v.) by means of a bridge, the variable branch of which consisted of a noninductive and noncapacity resistance and a variable capacity connected in parallel.

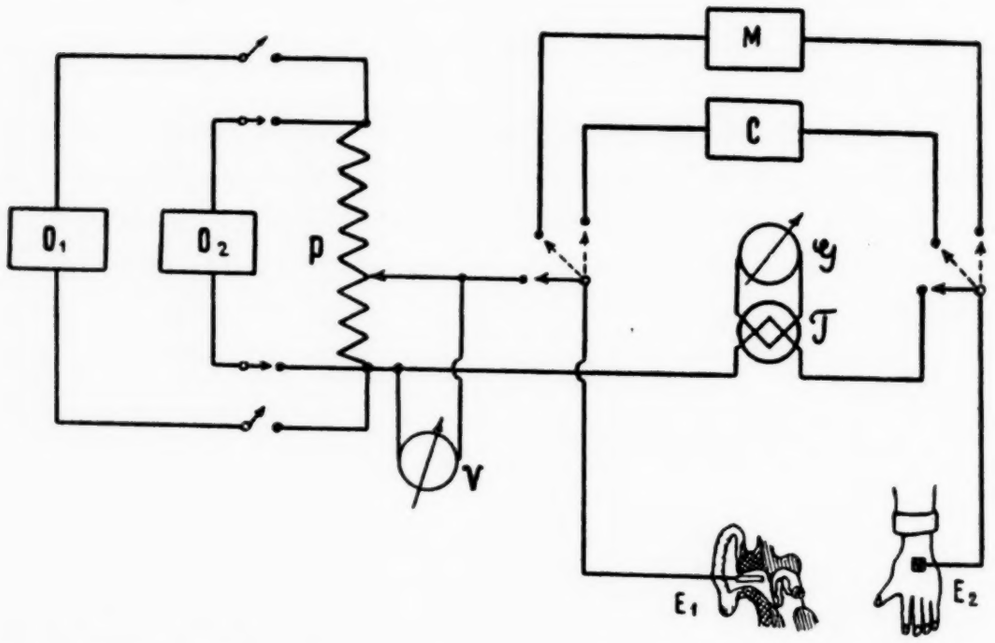

FIG. 1. O_1, O_2—oscillators; P—potentiometer; v—tube voltmeter; M—Whitston bridge; C—variable capacity; T—thermocouple; G—galvanometer; E_1, E_2—electrodes.

In the text the resistance of the body circuit is expressed by the ohmic resistance of the variable branch (r).

In order to obtain electric vibrations lower than 750 d.v., we used a heterodyne oscillator for low sound frequency which Prof. N. N. Andreef (Institute of Electrophysics in Leningrad) had kindly placed at our disposal.[1]

Results of Experiments.—The typical data obtained on one of the observers are given in Table I. As can be seen from the table auditory sensations arose with frequencies from 17 up to 14,000 d.v. The character of sensations with frequencies from 100 d.v. and higher is exactly the same as during an adequate stimulation. This fact was proved by frequently repeated comparisons of tone pitches, effected by means of an alternating connection of offsets from the oscillater to the body circuit or to the telephone, placed at the other ear of the person under investigation. Tones of the same pitch arose

[1] We wish to express our gratitude to Prof. N. N. Andreef for placing the apparatus at our disposal and for his many helpful suggestions.

together with the electrical and adequate stimulations. A distinct musical character of the sensations appeared with frequencies of the order of 100 d.v.

TABLE I

Cycles	Threshold Current Strength in m.A.	Character of Sensation	Cycles	Threshold Current Strength in m.A.	Character of Sensation
17	0.47	Short single noises in quick rhythm	750	0.14	Entire similarity of sensations during electrical and adequate stimulation as compared with the sound in the telephone
30	0.41	The same, shorter intervals	850	0.15	
60	0.22	Continuous humming noise	869	0.16	
96	0.32	Continuous humming noise with a musical character	1,064	0.13	
140	0.41	Very low tone	1,433	0.29	
203	0.30	Tone of higher pitch	2,000	0.24	
280	0.17	Beginning at 96 d.v. and higher an	2,200	0.47	
390	0.31	entire similarity of sensations	4,000	0.45	
550	0.08	during electrical and adequate	5,500	0.51	
670	0.09	stimulations, as compared with the sound in the telephone	14,000	0.78	

The phenomena with frequencies lying at the lower limit of audition (15–35 d.v.) proceeded in a different way. At the onset of electrical stimulation interrupted noises were observed. The phenomena were most marked at frequencies of the order of 15–20 d.v. In these cases the electrical stimulation caused single noises with distinctly marked intervals: whereas during an adequate stimulation no sensations were observed. It would be difficult to explain the fact of originating of those sensations otherwise than by the direct stimulation of nerve-elements of the auditory apparatus by the alternating currents. This explanation seems the more probable if we take into consideration that similar sensations are experienced by people, whose sound percieving apparatus (cochlea) is completely affected (Andreev, Gersuni and Volokhov 1935).

The great resemblance in the character of the sensations arising with frequencies above 100 d.v. during both electrical and adequate stimulations is proved by the possibility of the originaling of a binaural auditory image. The experiments were performed in the following way: the alternating current (200 d.v.) was conducted to the left ear; at the same time a

telephone from the same source of electric vibrations was placed close to the right ear. During such simultaneous stimulation of both ears a localized auditory image appeared. By varying the distance between the telephone and the ear we succeeded in obtaining a displacement of the image round the head. The disconnection of the telephone caused monoaural sensation from the left; the disconnection of the body circuit a monoaural sensation from the right.

It should be noted that the phenomena of entire conformity of the electrical and adequate stimulations described above were observed to the same extent in people whose middle ear elements had been entirely excluded. In such cases all the auditory sensations bore the character of a musical tone and entirely corresponded to the sensations observed during adequate stimulation (Andreev, Gersuni, Volokhov 1935).

Since the phenomena occurring during the flow of alternating currents through the uninjured apparatus of the internal ear are entirely similar to those observed during adequate stimulation, it is natural to admit that some portions of the receptor apparatus are stimulated in both cases in the same way. This is possible if, during the flow of alternating currents through the auditory apparatus some mechanical vibrations arise somewhere in the circuit and stimulate the cochlea in the ordinary way. Thus we may represent the auditory apparatus as being a sort of an electro-mechanical system which can transform electric vibrations into mechanical ones. This would be the easiest explanation of the phenomena observed.

On the Rise of Mechanical Vibrations.—We have attempted to bring forward some experimental evidence in favour of the opinion on the rise in the system of mechanical vibrations. For this purpose the following method can be used: If, indeed mechanical vibrations arise in the auditory-system during the flow of alternating currents, then if we simultaneously conduct sound vibration differing in phase or frequency to the same ear from outside, we can observe either beats or an entire extinction of sound, owing to the phenomena of interference.

The experiments were carried on in the following way: Alternating currents (800–1400 d.v.) were conducted to the ear. At the same time a telephone receiver from another oscillator was brought up to the same ear, the other ear being closely shut (Fig. 2). The intensity of sound in the telephone was so selected as to make the loudness (sensation level 20–25 db.) of the 'electric' and the 'ordinary' sound almost similar. The sound pitch in the telephone altered gradually. At a definite frequency the subject noted the appearance of

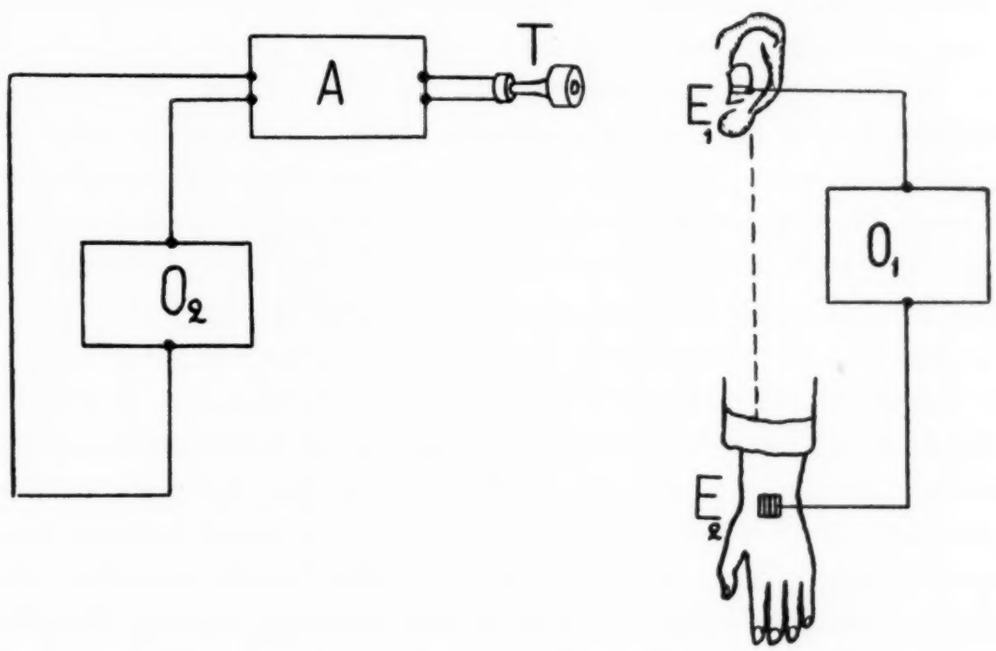

FIG. 2. O_1, O_2—oscillators; E_1, E_2—electrodes; A—attenuator; T—receiver.

beats. The frequency of the beats was calculated to be 2–3 beats per sec. The experimentator then perceived only the sound in the telephone. After finding the frequency at which the beats appeared, the body circuit was disconnected and a telephone was connected instead of it. All the persons present could then hear quite distinct beats. In several cases the telephone was connected parallel to the body and the beats were counted both by the subject and the experimentator. The rhythm of the beats coincided in both ways of listening. Such tests were performed many times in the presence of different observers, and secured always the same results.

The fact of the rise of typical monoaural beats with frequencies of the order of 800 and 1400 d.v., seems to us to be a very convincing proof of the possibility of a rise of mechanical vibrations in the ear system, during the flow of alternating currents.

Localization of Mechanical Vibrations.—If we admit the presence of an electro-mechanical system in the circuit of the auditory apparatus, we should be naturally confronted with the following problem: In which portions of the circuit do the mechanical forces arise? The solution of this problem seemed very essential. Indeed if we suppose that the electrode itself is a sounderadiator and that the mechanical forces are developed in it, that phenomena would present no physiological interest. In this case such an electrode might be compared with a telephone of a special kind, introduced into the external acoustic canal. But if the development of mechanical forces during the flow of the current takes place in some of the elements of the auditory apparatus itself, such a transformation of electric energy into mechanical would be of considerable interest for the analyses of processes occuring in the auditory apparatus. Admitting the possibility of a sounderadiation by the electrode itself we could suggest two variations. First, the vibration of the whole metallic rod during the flowing of the alternating current; second, the rise of vibrations on the line of contact between the electrode and the liquid.

The fact that the whole rod does not yield any acoustic vibrations was proved by the following experiment: a wire (of the same material as the electrode) made in a loop and covered with an insulating lacquer was introduced into the auditory canal of one of the subjects, while an ordinary electrode was inserted into the auditory duct of another subject. Both were connected in series to the circuit. Thus the auditory apparatus of the first subject was not electrically connected with the circuit, whilst the auditory apparatus of the second subject made part of it. If the electrode yielded acoustic vibrations, the sound would be heard by both persons, but if the vibrations appeared as a result of the current flow through the ear

circuit, the sound could be heard only by the second subject (Fig. 3).

The experiments showed that only the second subject experienced auditory sensations. The strength of the current in the circuit exceeded by far the usual threshold magnitude. After having scraped the lacquer of the wire, the first subject was connected parallel to the circuit, whereupon the sound was heard by both observers. These experiments bear undoubted evidence to the fact that there is no ground whatever to admit the vibration of the electrode itself.

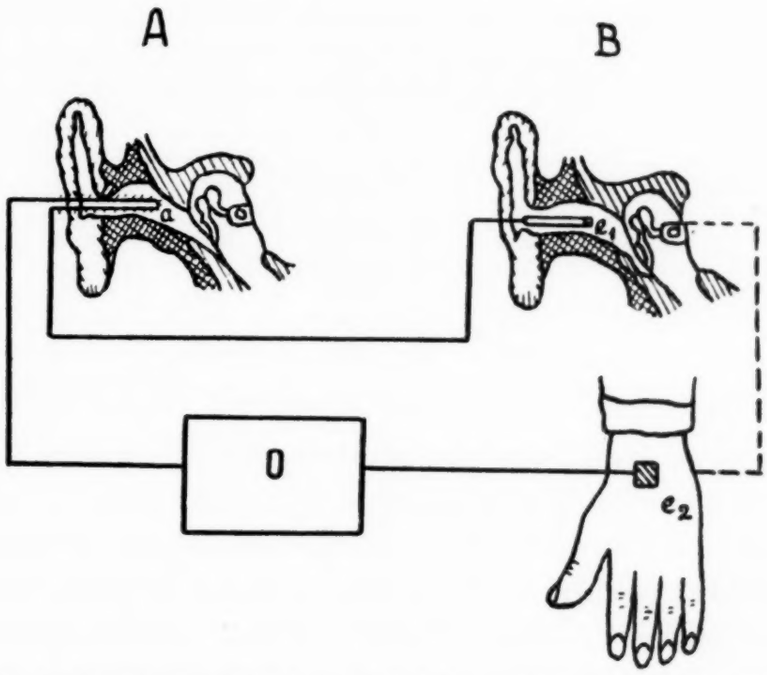

FIG. 3. *A*—the first observer; *B*—the second observer; *O*—oscillator; e_1, e_2—electrodes; *a*—isolated wire.

As to the second variation of the possible rise of vibrations at the electrode, it might be supposed that the development of alternating mechanical forces took place on the boundary line separating the metal from the liquid (electro-capillary phenomena).[2]

In order to solve the problem of the rise of vibrations at the electrode we performed a series of experiments. In these

[2] The possibility of the rise of acoustic vibrations on the line of contact of the metallic conoluctor with the dry skin, was proved by the experiments of E. Gray (1877), Jellinek and Scheiber (1930), Perwitzschky (1930), Kahler and Ruf (1931), Gersuni, Lebedinski and Volokhov (1934).

experiments we proceeded from the following considerations: if the electrode is a sounderadiator inserted into the auditory duct, we are sure to observe a vast difference in the threshold current strengths for different persons with a different loss of hearing, as it may likewise be found when measuring the current strength of a telephone.

Table II represents the threshold current strengths at a frequency of 1000 vibrations per sec for persons with different loss of sensitivity for a pitch of this frequency.

TABLE II[3]

Subject	Loss of Audition for Tone of 1000 d.v. in Decib.	Threshold of 'Electrical Audition' in m.A.	Threshold of 'Electrical Audition' in Volts	Resistance in Ohms (r)	I^2r (watt)
V-v	0	0.25	0.52	2000	$1.25\text{-}10^{4}$
U-a	60.0	0.10	0.21	2000	$2.0\text{ -}10^{6}$
B-v	60.0	0.40	0.57	1900	$2.24\text{-}10^{4}$
N-v	50.0	0.49	0.82	1260	$3.02\text{-}10^{4}$
G-y	0	0.70	1.16	1670	$7.89\text{-}10^{4}$
L-o	40.0	2.20	5.72	2860	$1.14\text{-}10^{2}$
S-a	70.0	0.96	1.50	1584	$1.42\text{-}10^{3}$

As can be seen from the table threshold current strengths are in no connection whatever with the loss of audition. Thus with the subject U-a whose sensibility is diminished millionfold, the threshold current strength is twice and a half less than with a normal subject. These phenomena indicate definitely that the transformation of electric vibrations into mechanical ones cannot take place in the electrode or in any other elementary sound eradiating system, which does not depend on the structures of the auditory apparatus themselves. Therefore it is impossible to speak of the fluctuation of the volume liquid, as result of Joules' heat discharge, for in this case a definite dependance between the heating power (J^2r) and the ear sensitiveness for the given frequency, would be required. Thus the vibrations do not arise in the electrode, neither are they the result of the vibration of the column of liquid itself (independently of the causes which may produce

[3] Part of the data upon which the reckonings of this table are based is taken from the work of Andreev, Gersuni and Volokhov (1935).

it); it remains to suppose that the vibrations arise owing to the activity of certain structures of the auditory apparatus. As these phenomena were observed after the contents of the middle ear had been removed, we may draw the conclusion that these structures are located in the internal ear (cochlea).

Discussion.—The phenomena that have been described above lead us on the whole to the following conclusion: if the inner ear mechanism is intact, alternating currents cause in certain systems of the auditory apparatus the development of a series of processes which lead to the rise of the whole range of auditory sensations. In order to explain the phenomena described above, we may admit the existence of a certain system transforming the electric vibrations into mechanical.

Undoubtedly this system is not located either at the electrode, or in the middle ear, or in the nerve tracts of the auditory apparatus. The latter is proved by the experiments with the stimulation of the nerve tracts of the auditory apparatus by alternating currents, performed on people with severe affections of the inner ear (see Andreev, Gersuni and Volokhov (1935). Therefore this system must be located in the inner ear and most probably in the cochlea.

How can we imagine such a system? Two suggestions may be advanced: (1) This energy transformation is a phenomenon which is the result of those physical processes, which are developed in certain parts of the cochlea and require no activity of the cellular structures of the cochlea for their manifestation. If so, we can draw a complete analogy between this system and the electromechanical systems known in physics (piezoelectrical, electrocapillar etc.) and we must try to find a scheme appropriate to this case.

(2) This energy transformation necessarily requires the activity of the cellular elements of the cochlea and we deal with a much more complicated biologic process. In this latter case the term 'electro-mechanical system' itself is to be understood very conditionally. It is not at all excluded that the current may influence directly those biologic processes which with adequate stimulation occur in the cells under the

influence of alternating mechanical forces. If so, the transformation of electric forces into mechanical ones may not take place in an energetic way. We have no experimental evidence at present which may speak in favour of this or other suppositions. Nevertheless, notwithstanding the interpretation of the nature of this phenomenon, a question arises as to the reversibility of processes in this system, *i.e.* whether a rise of alternating electric forces takes place during the acoustic vibrations of the cochlea. It is enough to consult the literary data on the electric phenomena arising in the auditory apparatus, to be convinced that the phenomena are reversible. Indeed, when the auditory apparatus is affected by sound, alternating potentials may be observed, which in their form and frequency reproduce the affecting sound (Wever and Bray (1930); Davis, Derbyshire, Lurie and Saul (1934) etc.). Thus we see that on the one hand, when conducting sound vibrations from without alternating electric potentials arise in the cochlea (Wever and Bray), on the other hand, when applying external alternating tension, acoustic vibrations arise in the cochlea, as our data indicate. We think, we could very well admit the supposition that both kinds of phenomena arise in the same reversible 'electromechanical' system of the cohlea.

We have discussed above the two possible mechanisms of this system. Davis and others proffer a series of data indicating that the presence of auditory cells is necessary for the rise of the cochlea potentials. The experiments of Adrian, Bronk and Phillips (1931) who observed a marked diminution of potentials during the cooling and cocainization of the cochlea, speak to a certain extent in favour of the biologic character of the system. On the other hand Hallpike and Rawdon-Smith (1934) pointed out the importance of the Reissner's and tectorial membranes. A further experimental study is however still necessary for all these data. It is very possible that the 'electromechanical' system of the cochlea includes a series of cellular and noncellular elements.

The problem of the physiological rôle which this 'electro-

mechanical' system plays in the processes of auditory perception remains as yet altogether unsolved.

Summary

1. The auditory sensations arising during the flow of alternating currents through an uninjured auditory apparatus, beginning from 100 d.v., entirely correspond to the sensations observed during adequate stimulation. Thus, all the frequencies up to those which lie at the upper limit of audition (14,000 d.v.) are quite distinctly perceived.

2. It is suggested that the sensations observed during the flow of alternating currents through an uninjured auditory apparatus are the result of the rise in the current circuit of mechanical vibrations which stimulate the cochlea in an ordinary way.

3. The mechanical vibrations do not arise either in the electrode inserted into the liquid, or in the middle ear cavity. Arguments are brought forward in favour of the possibility of the rise of mechanical vibrations in the cochlea.

4. The cochlea elements may be considered as some sort of 'electromechanical system' which transforms electric vibrations into mechanical ones. The problem of the physical and biological mechanisms which may be at the base of this system has been discussed. It has been pointed out that in the case when the cellular elements of the cochlea take part in the phenomena described, a direct effect of the current on the biologic processes developing in the auditory cells may be possible, without the transformation of the electric energy into mechanical. Therefore the term 'electromechanical' system should be adopted conditionally.[4]

(Manuscript received June 7, 1935)

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[4] We wish to express our gratitude to Prof. L. A. Orbeli for his constant interest and for his valuable suggestions while we were conducting this work.

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A DEVICE FOR MEASURING KYMOGRAPHIC RECORDS*

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Many forms of kymographic tracings obtained in psychological and psysiological work are of such an irregular, complex or curvilinear nature that their measurement is extremely difficult. Curves which register changes in level as in the case of plethysmographic tracings, or curves which show rhythmic fluctuations, like breathing curves, are of this character. Spontaneous pressure or bulb-squeezing records and so-called actograms of the Szymanski type, in many cases present particularly perplexing problems in this respect.

An apparatus called a 'linear oscillometer' designed for use upon irregular curves like those mentioned has recently been described by Hull.[1] Its specific function is to record perpendicular deviations from a base line. When used with kymographic records, this apparatus permits an operator to determine the extent of the excursions originally made by the kymographic writing points in a direction at right angles to the direction of rotation of the kymograph drum. Hull's instrument, therefore, fills a real need in scientific research. Perhaps its chief disadvantage is its size and bulkiness.

A smaller and simpler device for accomplishing the same result has recently been developed at Indiana University. The Indiana apparatus not only has the advantage of portability, but has the further advantage that it can be adjusted to compensate for, or eliminate from its measurements, the arc of the original kymographic writing point which traced the line being measured. It is built on the principle of a

* The development and construction of this apparatus were financed by a research grant from Indiana University.

[1] C L. Hull, An instrument for summating the oscillations of a line, this JOURNAL, 1929, 12, 359–361.

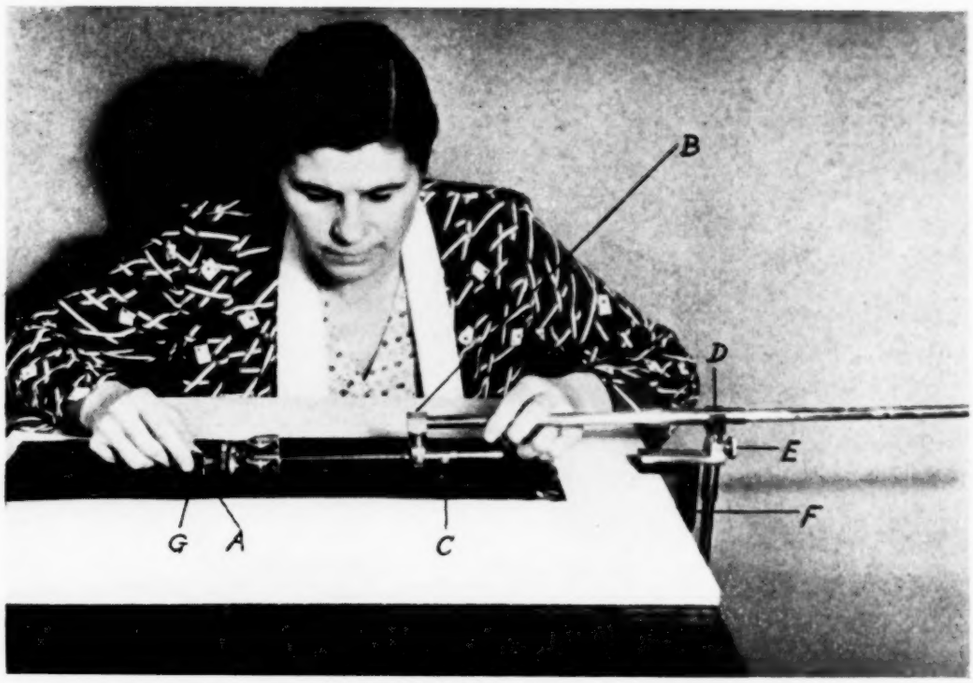
planimeter, and in fact may be briefly described as a combination of two modified planimetric units. Photographic illustrations are given in the accompanying figure.

In using the device (see photograph of instrument in use) the operator holds the pivoted guiding piece, *G*, between thumb and forefinger and causes the tracing point, *A*, to follow the line whose excursions are to be measured. The double (horizontal and vertical) pivot at *B* permits the tracing point to be raised or lowered as well as to be moved about in a horizontal plane. Beneath *B* is a point of contact which rests upon the kymographic record. This point serves as the center of a circle whose radius is the length of the arm *A-B*. When the tracing point, *A*, is moved, therefore, it rotates through an arc as most kymographic writing points do. The radius of this arc can be adjusted by the set-screw, *C*, so as to make it the same length as the radius of the original writing point. Any distortion in the kymographic tracing which was caused by the arc of the marker is thus counteracted or eliminated by having the same arc in the measuring device.[2]

The mechanism slides to the right and left by means of the guiding sleeve at *D* which can be raised or lowered (using the set-screw, *E*) and which also has a pivotal connection so that it can be tilted. A rigid clamp, *F*, fastens the whole apparatus to a table or drawing board.

The measuring parts themselves, shown in the close-up photograph, consist essentially of two recording discs, *I* and *I'*, which press upon the kymograph paper and rotate in opposite directions. (The tracing point, *A*, is not visible from the angle of this picture, but its position is indicated by an arrow.) When the operator moves the mechanism in a direction towards the reader, *I* rotates, and *I'* remains fixed. This is accomplished by the pawls *J* and *J'* which are activated by the wire spring *K*. If the device is moved to the right or left (as shown in the picture) neither disc rotates. If it is

[2] This method of construction necessarily introduces a small error of measurement in records which have no original arc themselves, as for example in oscillographic or other types of photokymographic records. By lengthening the arm *A-B*, however, the distortion introduced by the arc-like movements of *A* may be reduced to such an extent as to be negligible.



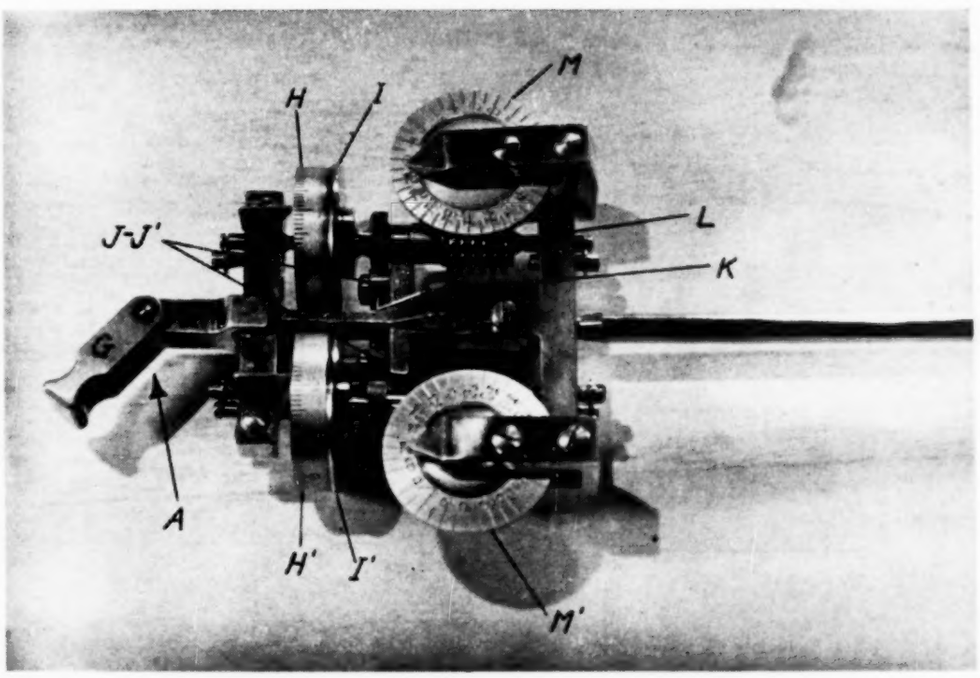


PLATE I. Showing construction of the Indiana graphometer or lineometer, and the method of using this instrument.

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moved in a diagonal direction, one disc only rotates in proportion to the perpendicular deviation from the horizontal (right-left) line. By following an irregular line for a given interval, say 10 seconds of kymographic tracing, one can obtain from one of the recording discs, the sum of the deviations *in one direction* from the base or horizontal line. The other recording disc then summates the deviations *in the opposite direction*. Subtracting the two sums gives the general trend of the line within that 10 second interval. Adding the sums gives the total amount of activity of the kymographic marker during the time interval included.

Fine decimal scales, *H* and *H'*, fastened to each of the rotating discs permit accurate readings of the deviations. Worm gears (*L*) transmit the motion of the discs to the gross scales, *M* and *M'*, which are in a 10 to 1 ratio with the decimal scales. The units of measurement are centimeters on the gross scales and millimeters on the decimal scales.

This apparatus has been in use at Indiana University for more than a year, and although not always perfectly accurate has proven invaluable in getting some system from irregular and 'jiggley' kymographic tracings. We rather hesitate to call the device a 'linear oscillometer' after Hull, but would suggest some less formidable term, such as 'graphometer' or 'lineometer.'

(Manuscript received January 30, 1935)

A MULTI-MULTIPLE-CHOICE MACHINE FOR EXPERIMENTAL WORK WITH REWARDS AND PUNISHMENTS[1, 2, 3, 4]

BY IRVING LORGE

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AND

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For nearly a decade, Thorndike and his students have been evaluating the efficacy for human subjects of rewards and of punishments. The primary findings to date have been, first, that multiple choice learning proceeds essentially by the influence of reward; second, that punishment does not offset entirely the strengthening due to the sheer occurrence of wrong connections; and third, that a reward strengthens not only the connection it immediately follows, but strengthens also unrewarded connections proximate to the reward.

The third finding is of considerable theoretical import. Much experimental work is necessary, however, to determine the nature of the influence of a reward upon connections proximate to it. Among the problems awaiting solution is that of ascertaining whether the 'spread' or 'scatter' influence of a reward operates upon connections proximate in time, or upon connections proximate in sequence.

The general pattern of the reward and punishment experiment has been that of the multiple-choice situation. The

[1] Acknowledgment is hereby made of the services rendered by the personnel furnished by the Works Division, Emergency Relief Bureau of New York City, on Project 89FB—125X.

[2] We wish to thank Remington-Rand, Inc., for supplying the discarded typewriters which were used in the construction of this machine.

[3] We wish to thank Adam Kunze of the Kunze Typewriter Co. and William Hawes of Hawes and Petit for technical advice in designing the recording system.

[4] This machine was built in connection with a study supported by a grant from the Carnegie Corporation.

typical procedure has been to present a series of situations to which the subject makes one response from among many to each situation. Each response to each situation would be rewarded, or punished, or neither rewarded nor punished, by the experimenter. As soon as the response to a stimulus or situation had been made, the situation would vanish, and as soon as the experimenter had delivered the after-effect, another situation would be presented. The procedure would be continued for all the stimuli of a series for a specified number of trials. This procedure was limited to experimentation with one individual at a time. It was very costly in terms of experimenter's time. At best, a single experimenter could work with but eight subjects a day.

Adequate information concerning some of the subtler influences of rewards requires at least the data from six hundred subject-hours of experimentation. To collect so much data, the individual experimental procedure was adapted to group work through the means of the multi-multiple response machine. The machine, basically, consists of an exposure apparatus for presenting the stimuli, and reaction keys for choice responses. Thirteen typewriters, one for each of thirteen subjects, provide the reaction keys which served three purposes: response to the stimuli, recording the responses, and administration of the rewards, punishments, and ambiguous after-effects.

Diagram 1 gives the plan of the machine. Thirteen typewriters are arranged in pairs, back to back, with the exception that the thirteenth typewriter is backed by the motor mechanism. The typewriters are stripped of all non-essentials (such as ribbon, keys, shift keys, and space-bar), with the exception of the five keys, F, G, H, J, and K in the center of the keyboard. The five keys of the typewriter are the five choice reaction keys of the experiment. The platens of the typewriters on either side of the table are coupled for synchronous movement. In the center of the table is hung the main shaft to which the platens are connected in a sprocket and chain drive. The movement of the main shaft will bring synchronous movement of the platens. The main shaft is

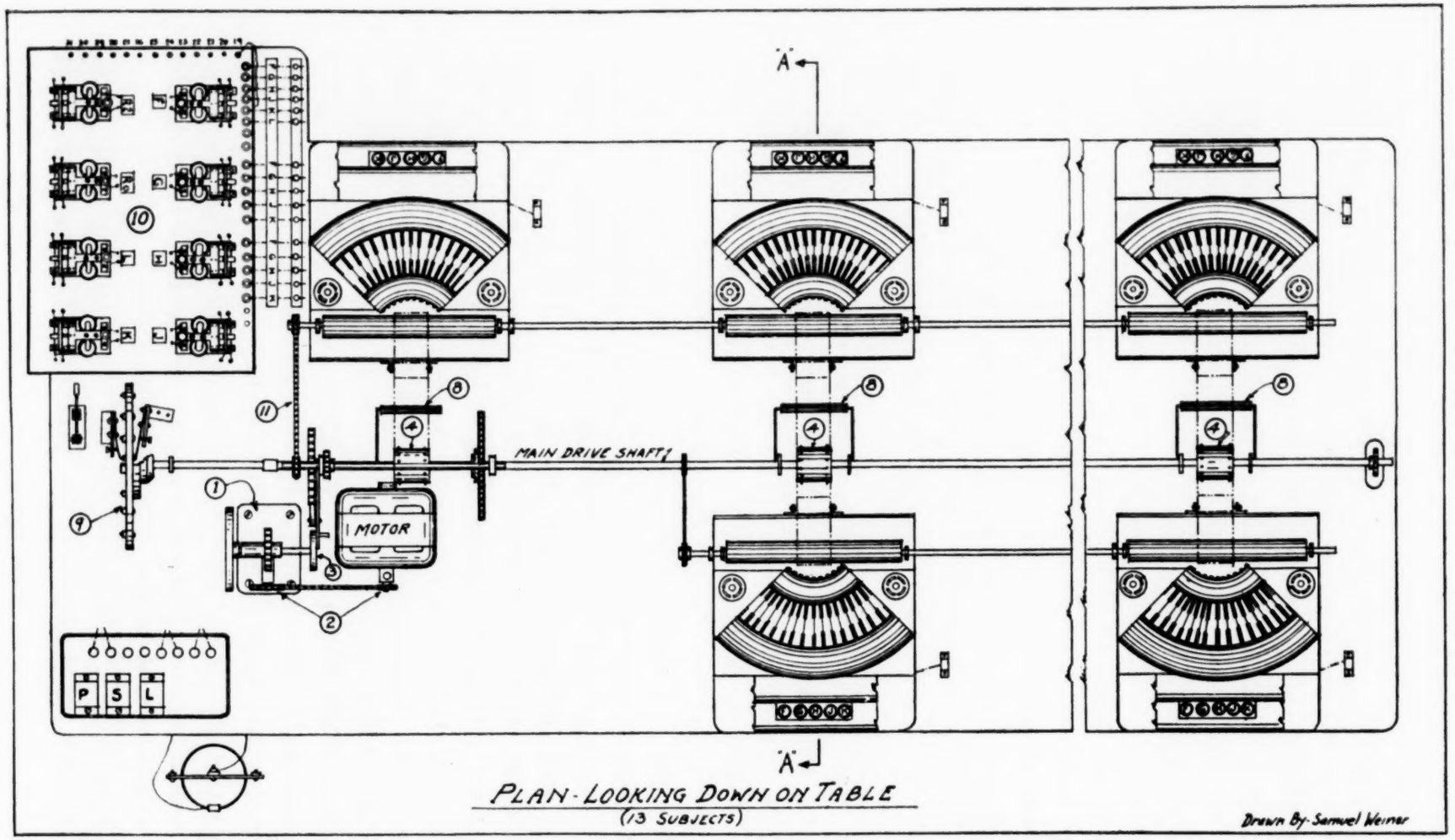

DIAGRAM 1. Plan of the multi-multiple-choice machine. The numbers in all the drawings for this machine have the following meanings: 1, reducing unit; 2, sprockets to vary time of presentation of stimuli; 3, cam, lever and pawl; 4, toothed wheel to drive stimulus tape; 5, retention rollers; 6, window in which stimuli are framed; 7, pilot-light illuminating stimuli; 8, idler roller; 9, control disk; 10, relay board; 11, chain drive to typewriter platens; 12, auxiliary shaft to regulate tape.

powered by a motor operating through a reducing unit to a lever, pawl and ratchet which operate to turn the main shaft through 1/40 of a revolution for each complete revolution of the output of the reducing gear. By changing the ratio of the teeth on the sprocket on the motor to the teeth on the input of the reducing box, the time intervening between each 1/40 turn can readily be changed.

The stimuli were presented to each subject in a little window at his eye-level. The stimuli were drawn or typed on strips of heavy manila paper approximately 2½ inches wide. The paper was perforated along each side much in the manner of motion-picture film. The paper for any pair of machines was passed through retention rollers behind each window and over a toothed roller, and pasted into a continuous strip. The toothed roller was on the main drive shaft, so that for each 1/40 revolution of the main drive shaft, the paper strip was moved 1/40 of its length, *i.e.* one stimulus frame. Diagram 2, the elevation of the machine, shows the location of the window before each subject. Immediately above each window, a shielded pilot light illuminates the stimuli during the course of the experiment. Diagram 3, a section through line A–A, illustrates the manner in which the stimulus strip is threaded through the retention rollers before each window. The same strip serves as stimuli for each of two subjects. When the stimuli were nonsense symbols, the subject on either side of the strip responded to the same stimuli at 180° rotation to each other. When the stimuli were nonsense words or meaningful words, the words were typed twice in each frame, one word 180° to the other. The subject was required to respond to the underlined word. When the subject on one side of the machine was reacting to the first stimulus of a series, the subject on the other side was responding to the twenty-first stimulus.

The subject made his response by striking one of the five keys before him for each stimulus. The key thus depressed would strike the recording paper passed over his platen. The recording paper was double-carbonized adding machine tape. Since the ribbon of each typewriter was removed, the subject

tion rollers; 6, window in which stimuli are framed; 7, pilot-light illuminating stimuli; 8, idler roller; 9, control disk; 10, relay board; 11, chain drive to typewriter platens; 12, auxiliary shaft to regulate tape.

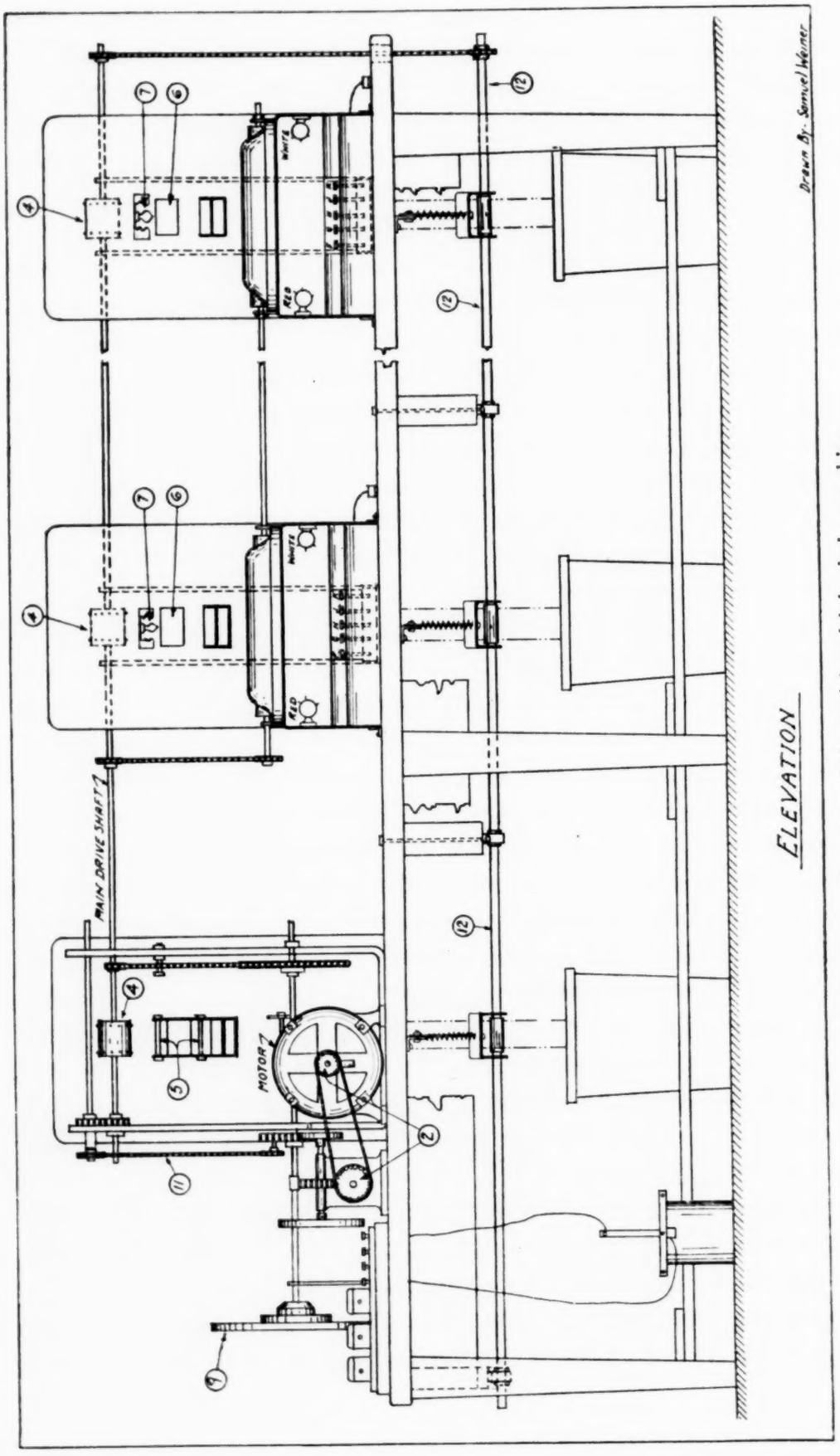

DIAGRAM 2. Elevation of the multi-multiple-choice machine.

could not see his response for review. The carbon on back of the first sheet, however, recorded the subject's responses on the second sheet. As the main shaft moved through 1/40 of a revolution, the platens also moved to space-up the record

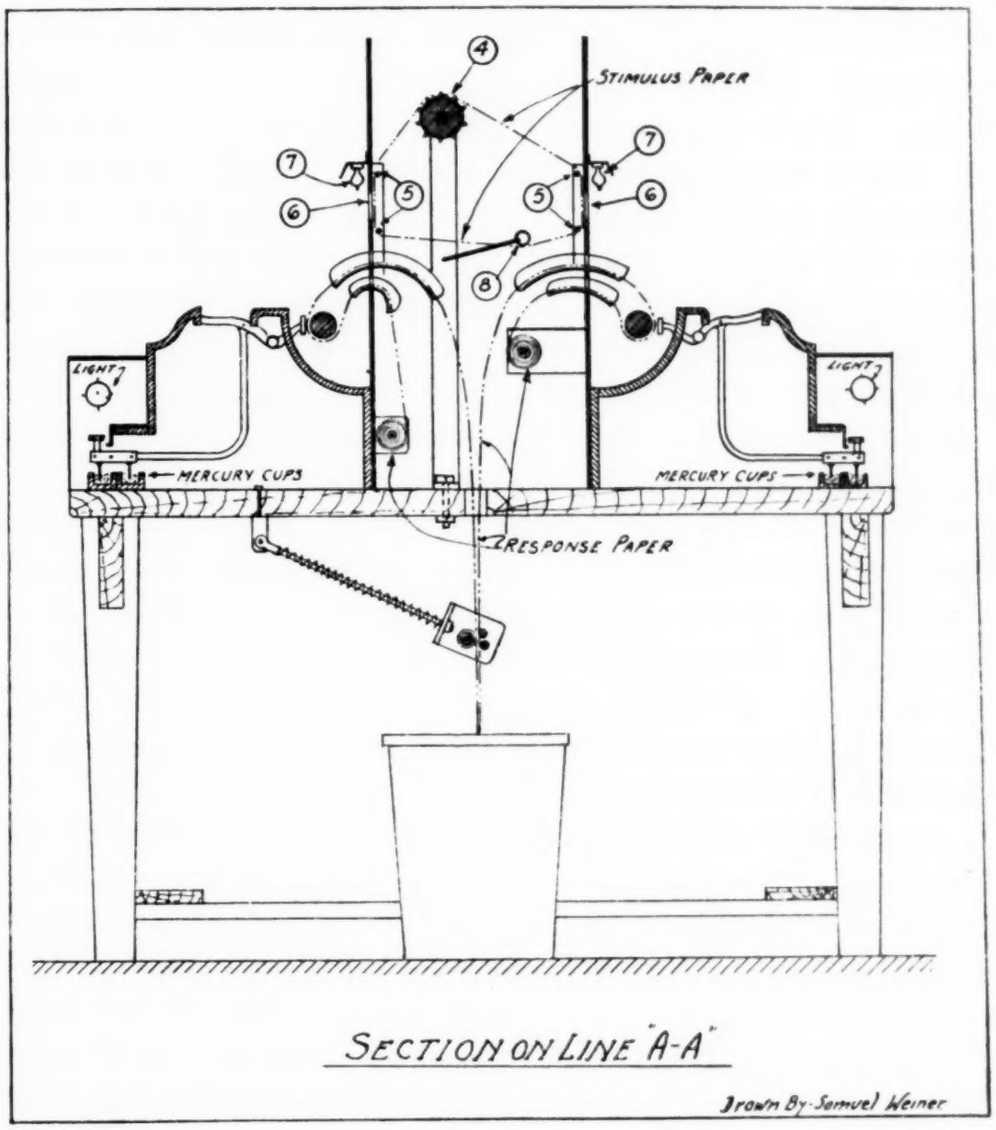

DIAGRAM 3. Section of the multi-multiple-choice machine through line A-A of plan.

roll. The record could then be synchronized with the presentation of the stimuli, and with the arbitrary distribution of rewards, punishments, and ambiguous after-effects. In diagrams 2 and 3 is shown, beneath the table, an auxiliary roller-system to synchronize the rate at which the recording tape is fed through the platen. The operation of the platens,

however, was so efficient, that the auxiliary rollers were not essential to the proper spacing of the tape.

Thus far, the mode of presentation of the stimuli, and the method of recording each subject's responses thereto, has been described. The after-effects were administered (diagram 2) by two small electric-light bulbs on either side of the reaction keyboard. The lamp on the left was red, the one on the right, white. When the response was arbitrarily pre-determined to be correct, the white light would flash; when the response was arbitrarily pre-determined to be wrong, the red light would flash; and when the response was arbitrarily pre-determined to be neither right nor wrong, neither light would flash. In this manner a white light served to reward, a red light to punish, and no light to leave ambiguous, each response.

Each reaction key bore two pins insulated from each other. Upon depressing any key, the pins would be dipped into two troughs of mercury (diagram 3), closing certain arbitrarily chosen circuits. The choice of circuits was governed [5] by the control disk shown in diagrams 1 and 2, and in detail, in diagram 4. The control disk was, in effect, a master multiple switch which governed the operation of a series of relays through which the rewards, punishments, and ambiguous after-effects were delivered.

Each relay served one reaction key. Thus five of the relays served to control the F, G, H, J, and K reaction keys. Each relay used two points: the 'off' position (no current flowing through the coils) was set for punishment; the 'on' was set for reward. To reward the response of 'F' for a stimulus, for instance, it was necessary to send current through the coils of the 'F' relay to throw the relay to the 'on' position during the time the stimulus was visible. If the subject struck the 'F' he was rewarded by the white light as long as the pins on the key remained in the mercury cups. If the subject struck any key other than 'F' the red light flashed since all other relays were in the 'off' position. When,

[5] Although eight relays are shown in diagram 1, only six were used in the multi-multiple-response machine. The relay board was an interchangeable unit serving this and another machine in which relays, R_0 and R_2, were required.

however, the response was to be ambiguous, relay *L* was required. In the 'off' position relay *L* was in series with the common lead to the other relays and with the current source for punishment. The 'on' position was a dead point which made no connection. Thus if relay *L* were thrown to the 'on' position the source of current for punishment was broken.

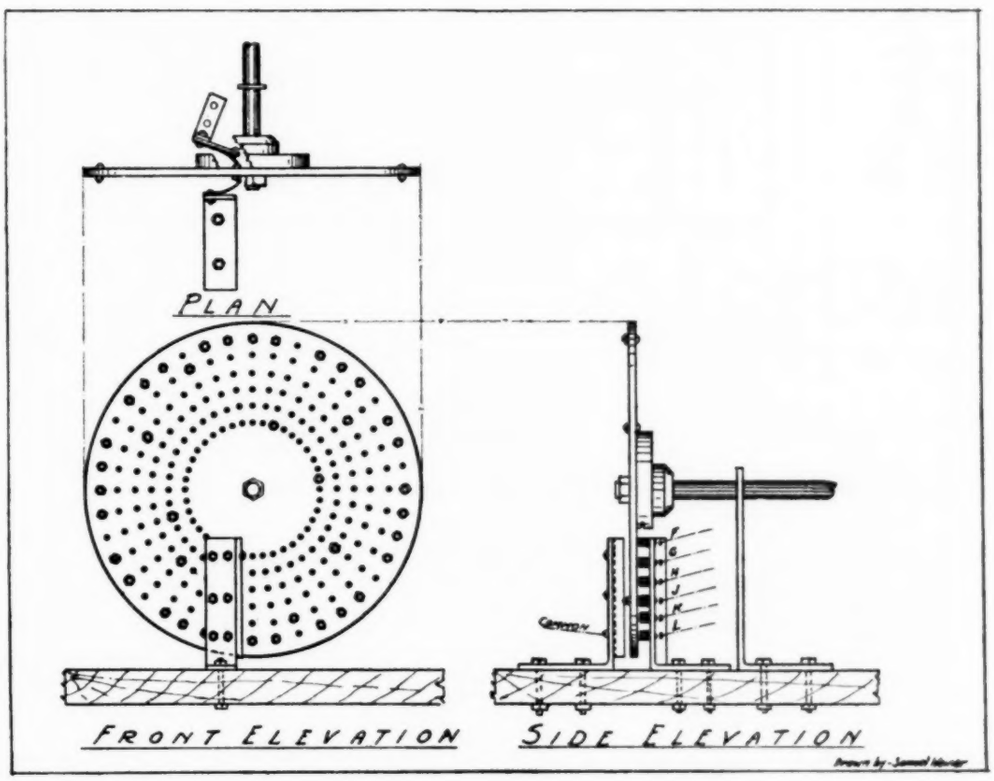

DIAGRAM 4. Detail drawing of control disk.

Since none of the other five relays were in the 'on' position no reward was possible. When the subject depressed a key under these conditions, no circuit would be made hence neither reward nor punishment was delivered for his response.

The relays were selected for operation by the control disk. The control disk was of bakelite one-quarter inch in thickness and about ten inches in diameter. It was drilled in the center with a one-half inch hole to lock on to the shaft of a face plate. As may be seen from either diagram 1 or 2, the face plate was fixed on a shaft geared to the main drive shaft so that the control disk would turn 1/40 of a revolution each time a new stimulus was presented to the subject. Each

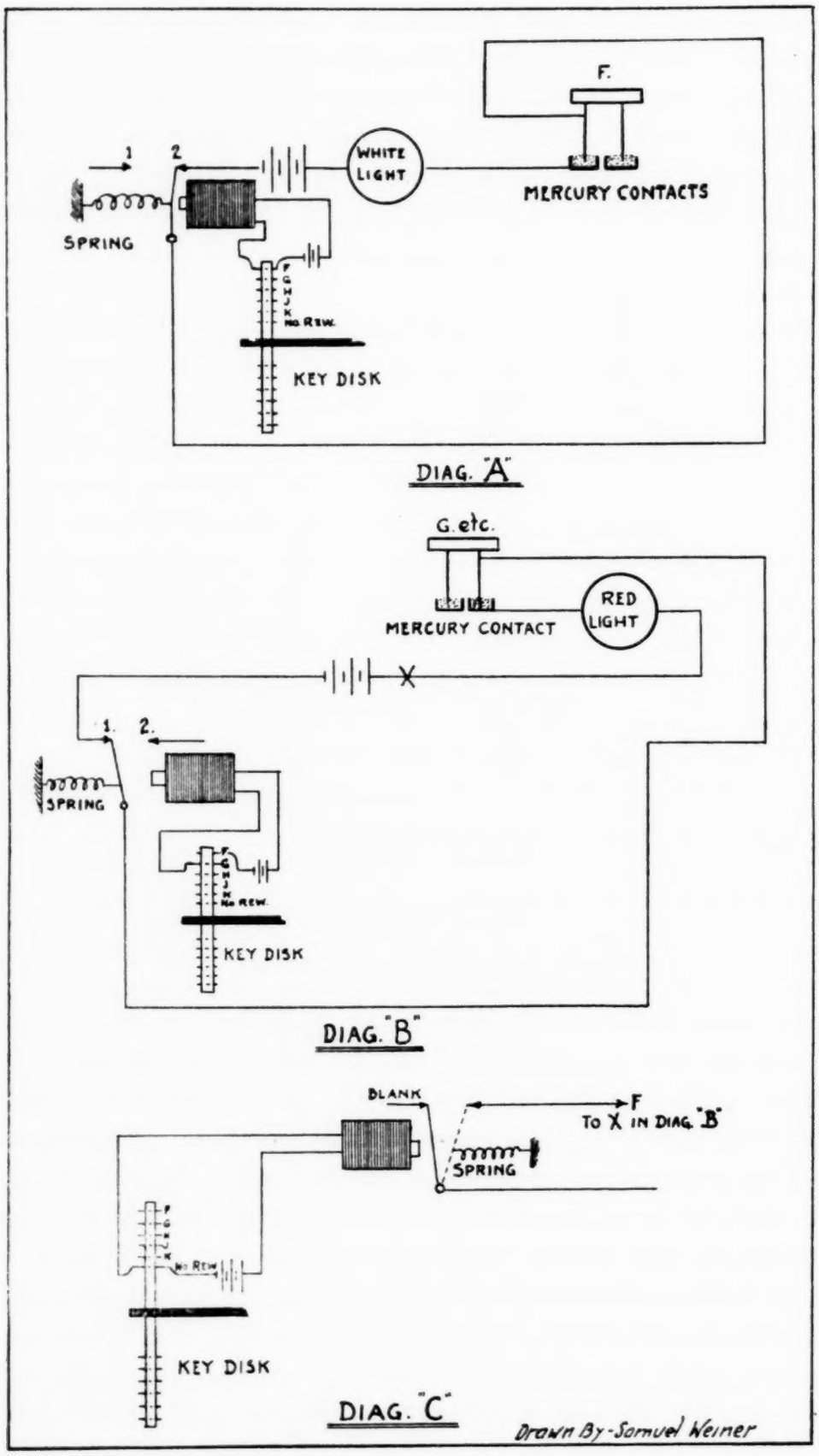

DIAGRAM 5. Schematic representation of the three possible electrical circuits.

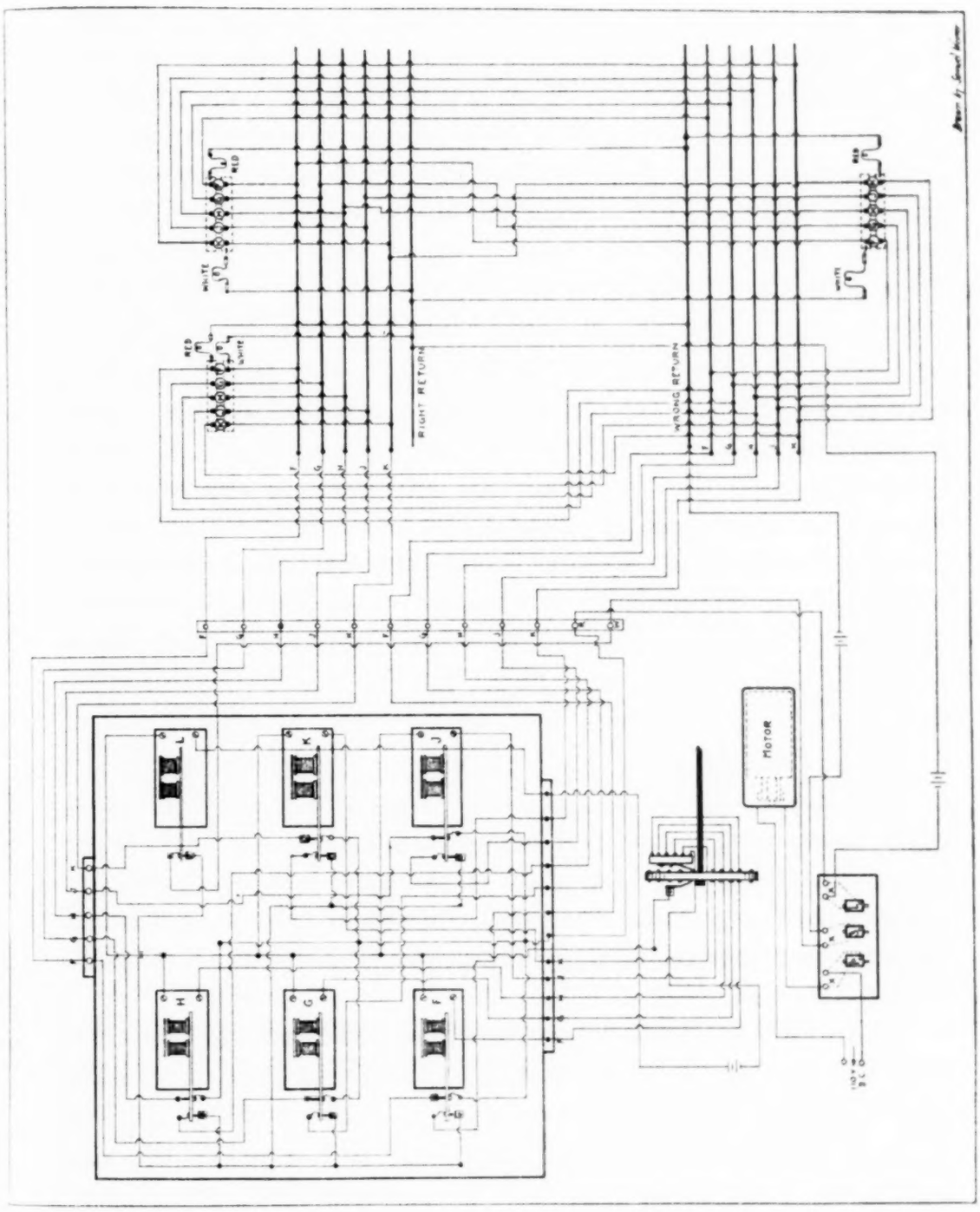

DIAGRAM 6. Wiring diagram for the multi-multiple-choice machine.

1/40 revolution brought a new key for the new stimulus presented to the subject.

The detail drawing of the control disk shows that six series of 40 equally spaced holes were drilled in concentric circles. Each concentric circle of holes mastered one relay which controlled one reaction key. Two sets of 'fingers' were mounted to make contact through a short brass screw through the control disk. The fingers of spring steel were mounted on bakelite, and were fitted with set-screws for adjustment of the tension. To throw any relay a screw was inserted to complete a circuit through the disk from the finger in front of the control disk to the one in back. To illustrate, if 'F' were to be 'right' for the first stimulus a screw was set in the first hole in the 'F' (inside) circle. This completed a circuit from a source of current through the disk, through the 'F' relay (throwing it to the 'on' position) and back to the source of current. Each stimulus thus had its key in the control disk.

The distribution of rewards, punishments, and ambiguous responses was determined by chance. Ten control disks with different chance arrangements were prepared. The leads from the control disk were fitted pin jacks. By rearranging the leads from one jack to another, 120 combinations of rewards, punishments, and ambiguous responses could be made without changing the disk. Diagram 5 shows a schematic drawing of each of the three possible after-effects. Drawing *a* illustrates the operation of a reward; drawing *b*, the operation of a punishment; and drawing *c*, the operation of an ambiguous after-effect. Diagram 6 gives the complete wiring diagram.

The multi-multiple-choice machine has been used successfully for over one hundred hours. By means of it, data have been collected from fifty-two subjects, each of whom worked at least twenty-four hours. Not only did it make the collection of data easier and less time-consuming, but it also eliminated the variable factor of the experimenter. Presentation of stimuli, recording of responses, and administration of the after-effects were entirely machine controlled.

(Manuscript received May 8, 1935)

DISCUSSION

A NOTE ON THE CONDITIONING OF VOLUNTARY REACTIONS

BY JAMES J. GIBSON

Smith College

J. M. Stephens has recently described experiments (1, 2) which at first sight seem to demonstrate that a voluntary reaction can be *conditioned.* Although the purpose of his experiments went beyond this demonstration, its validity underlies his main conclusions. Because of the importance of such a possibility for the theory of conditioning it is worthy of a rigorous experimental test. This it has not yet received.

In the most recent of his experiments (2) Stephens had his subjects react with the right hand as quickly as possible whenever a certain buzzer sounded and with the left hand whenever another buzzer sounded. The reaction consisted of hitting at contacts with a stylus. These two reactions were first practiced independently of one another, the buzzer-stimulus for one hand being presented a number of times and then the buzzer-stimulus for the other. Apparently no preliminary trials were given in which S did not know which stimulus to expect. Then about 100 paired presentations were run off, the two signals and the two reactions occurring either together or in close temporal succession. Apparently no 'ready' signals were given; the subjects were set to react throughout the experiment. At intervals during this series of concurrent reactions, tests for conditioning were made. That is, without warning, only one of the two buzzers was sounded. The criterion of conditioning was "that the subject when receiving only one signal shall move both hands or at least the hand for which no signal has been received" (2, p. 83). In this setting 'conditioned reactions' were obtained in a certain proportion of the test trials. Averages varied under different conditions of the experiment between 30 percent and 60 percent.

It is clear that under the circumstances described the subject may have been giving not conditioned reactions but only what would be called *false reactions* in the conventional choice-reaction

procedure. A reaction of the hand for which no signal has been received may mean (to an extent which is indeterminate in Stephens' experiment) only that when the subject is set to make either of two prepared reactions he may make both or the wrong one. The occurrence of these false reactions might have been as frequent before the conditioning procedure had begun as afterwards. The practice trials given before the beginning of the experiment were not choice reactions and consequently provide no control. True conditioning would be demonstrated only to the degree that false reactions increased, as a result of the training, beyond the frequency obtained in a control series. In other words genuine conditioning may be taking place in an experimental setup like the one described but it will be masked by the tendency shown by any prepared reaction to 'go off' to an inappropriate stimulus. This tendency is analogous not to conditioning but to the phenomenon of *irradiation* of the conditioned response. In the ordinary conditioning experiment the capacity of the conditioned stimulus to evoke the response before training is either well below the response threshold or is relatively slight; in the experiment employing voluntary reactions this capacity may well be fairly strong to begin with. A controlled procedure is even more than ordinarily necessary. Stephens' earlier experiment (1) is subject to essentially the same criticism in this respect.

There will be good reason to conclude that voluntary actions can be conditioned when it can be shown that a voluntary reaction to a specified stimulus is more frequently evoked by a non-specified stimulus in subjects given a conditioning series than it is in a group without such training. By a voluntary reaction is meant one dependent on a preparatory set aroused by verbal instructions. A non-specified stimulus is one which the instructions make inappropriate to the reaction in question, and to which ordinarily there is a set not to give that reaction. By a conditioning series is meant a number of paired presentations of the non-specified stimulus in conjunction with the specified one; as a parallel to this the control group might be given presentations of the specified stimulus *not* in conjunction with the other. An experiment of this type might employ the procedure of either the discrimination-reaction or the choice-reaction.

If responses of the type used in the reaction experiment are subject to conditioning of the type found in the reflex experiments much of the speculative use of conditioning to explain the formation of complex habits is given an added plausibility. The problem, therefore, has a considerable degree of importance.

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REPLY

BY J. M. STEPHENS

The Johns Hopkins University

Mr. Gibson is right in pointing out that to demonstrate conditioned voluntary reactions one should employ some such control as he suggests. I shall hasten to provide such a control so that my data will have more bearing on that particular problem.

Although I was not interested in showing that voluntary reactions could be conditioned, I find that one of my conclusions did make claims in that direction. Conclusion number 1 of my study (Stephens, **2**, p. 89) reads thus: "Both types of conditioning will appear to some extent." In view of Mr. Gibson's criticisms I realize that I should have said, "In no case did pure uni-directional conditioning appear." My fault is all the greater in that the second form of the statement would have served my needs much better. My interest throughout the series was in the *direction* of the conditioning, which matter is dealt with in conclusions 2 and 3. Conclusion number 1 was not necessary for my main purposes, but before going to conclusions 2 and 3, I wished to make it very clear that I did not find any one type of conditioning predominating to the complete exclusion of the other type.

I would like to leave the matter in such a state of unqualified agreement. I must, however, take exception to a point in Mr. Gibson's second sentence, since it may be misleading. If by 'main' conclusions he means my conclusion number 1, I am still in agreement as I have indicated above. If, on the other hand, he means my conclusions regarding the direction of conditioning (which from my point of view were my main conclusions), then his second sentence would appear to be in error.

Mr. Gibson has shown that the general conditioning or false reactions which I report (*a*) could have existed prior to the experiment or (*b*) could have been induced by the general readiness to act which characterized the subjects. But while either (*a*) or

(*b*) may account for the fact that *some* conditioning took place, neither can account for the specific nature of the conditioning which did take place—the fact that I got more of one type of conditioning than of the other. If *the preponderance of one type of conditioning* were due to either of these factors (*a* or *b*) such preponderance should have appeared equally in all five groups. All groups should have been affected by (*a*) preëxperimental preponderance and by (*b*) the general conditions of the experiment. Yet there was practically no preponderance of any one type of conditioning in the two groups which lacked a time interval but were subject to all the general conditions.

To my mind I have unquestionably shown (apart from the possibility of pure chance differences) that the *direction* of the conditioning is affected by the specific experimental factors. Just what bearing this finding has on the general possibility of conditioning voluntary reactions, I do not know. I would prefer to see that problem attacked directly.

There are two matters of procedure regarding which I did not make myself sufficiently clear. There were no 'ready' signals. From general observation and inference I would assume that a high degree of 'set' existed throughout the experiment. There were preliminary trials in which the subject was not *told* what to expect. In some of these trials such as the first one to each hand, I don't see how he would know what to expect. In other trials he quite probably could anticipate the signal. Let me add that this last is merely a quasi-factual statement and is not intended to refute Mr. Gibson's general argument.